Visual Attention:
An Action-Oriented Perspective

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Abstract

The dominant approach to the study of visual attention seems to rely on the use of highly abstract and constrained tasks to generate data which are then generalised to attention as a whole. The theoretical underpinnings of this approach are critically examined, and it is argued that its fundamental assumptions are flawed. An alternative formulation, the *action-oriented approach*, is presented as an alternative to the traditional approach. The action-oriented approach characterises perception and action as reciprocal parts of the same process, and views them as comprised primarily of highly specialised cognitive modules. It is argued that because previous research has not taken this structure into account, it can not be assumed that the results of such research are generalisable to actual performance in the real-world. To do this, attention must be studied using natural, real-world tasks. Two experiments were performed to examine how performance changed when a task common in attentional research, responding to a target in the presence of a distractor, was transformed from a highly artificial task using abstract stimuli and responses to a more natural task. This was accomplished in Experiment 1 by using cubes as the target and distractor stimuli, and having the participants respond by picking up the target object. The results obtained differed markedly from those found in previous research. In particular, response time measures were not quicker when the target was presented by itself. The results suggest that the participants engaged in a visual search for the distractor prior to initiating movement in order to avoid dislodging it when reaching. Experiment 2 attempted to test this hypothesis by replacing the distractor cube with a flat piece of copper that could not be dislodged. The instructions given to the participants were also varied, with some participants being told to avoid the distractor, while the rest were simply told to ignore it. The results indicate that, regardless of the instructions given, the participants ignored the distractor. These experiments provide an example of how the findings from abstract tasks can not be assumed to be generalisable to more naturalistic situations, as well as suggesting several interesting lines of research to pursue.

Introduction

It has been pointed out by some observers that Psychological Science, and Cognitive Psychology in particular, are largely based on the results of research which is very artificial. In particular, Ecological Psychologists such as Braddon (1986) and Michaels and Carello (1981) have argued that the experimental paradigms used in Cognitive Psychology are overly simplistic, that they lack *ecological validity*. In other words, the experiments Cognitive Psychologists perform are not adequate analogues of real life situations, and so are limited in the extent that they can be generalised. Since the experiments fail to capture key elements of these situations, the theories based on these experiments will also be flawed.

This next section explores the 'typical' approach within research methodology in visual attention with regard to the question of ecological validity, and argues for an alternative proposal, the *action-oriented approach.*

The 'Typical' Approach to Visual Attention

Scientific progress has always been based on the steady accumulation of knowledge which informs theory construction. Concomitant with this approach is the belief that an examination of any phenomenon should begin by using an extremely simple experimental analogue of that phenomenon in a natural setting and then advancing to progressively more complex and accurate analogues as knowledge increases. This can be achieved by breaking down the phenomenon under investigation into simpler subparts which can be studied independently. Progress in knowledge and the veridicality of theories built on this knowledge go hand in hand with the increasing real-world validity of the experimental analogues used[1].

Oddly enough, early researchers on attention, such as Broadbent, Moray and Treisman did not adopt this approach. An explanation of this may lie in the intent behind their research. Broadbent, for example, became interested in performance limitations associated with attentional selectivity through applied research with air traffic controllers (Anderson 1995). The experiments he and other researchers at the time designed were therefore directed largely towards a real-world problem. Consequently, the experiments they designed were good analogues of the real-world situation under investigation. This turned out to be a temporary phase, however. As a more abstract and generalised interest in the nature of attention became prevalent and technology developed, the experimental analogues used became increasingly abstracted from the real-world phenomena they sought to model. They also began to treat the perception and response phases of a task as logically independent. One could be studied quite apart from the other.

Since then, the methodology used to study visual attention has remained essentially unchanged. Likewise, our ideas and conceptions of attention have not materially altered. The same debates about early versus late selection and the capacity of attention are still just as lively now as in 1970. As Allport (1993) observed, if a Rip van Winkle of Cognitive Psychology were to awaken after 25 years of slumber, he would be struck by how little had actually changed in the field of attention during his absence. This begs an obvious question: If progress is supposedly being made in the study of visual attention, why has so little changed?

The answer, of course, is that very little substantive progress has occurred. While we know more about attention than we did, the empirical data has not led to the clarification of dominant issues such as the locus of selection and attentional limitation. This lack of progress is quite at odds with the growth experienced in other fields of Cognitive Psychology (Allport 1993). Likewise, the unchallenged longevity of some of Broadbent's original assumptions on the nature of attention is also curious. This suggests that one of the key reasons for the lack of progress is that these underlying assumptions are wrong. As Allport (1993) put it, 'Have we been asking the wrong questions?' To answer this, these questions need to be evaluated.

Consistent among researchers in attention have been a few postulates which have provided a starting point for any theorising on attention:

1. The human cognitive system is limited in some way in its ability to process all of the information provided by the sense organs, or there exists a bottleneck at some point in the flow of processing. Attention is a mechanism to protect the limited capacity system (Allport 1989, 1993; Broadbent 1958, 1971; Neumann 1987, 1990).
2. Related to the above point, that processing is essentially serial in nature, and that consequently selection can be located somewhere in the sequence of processing stages, be it early or late (Neumann 1987, 1990; van der Heijden 1992).
3. Attention is an unitary entity (Allport 1989, 1993).

Each of these assumptions will now be critically evaluated.

Limited Capacity as an Explanation of Attention

That the human cognitive system has a limited capacity to process information has been almost axiomatic in theories of attention. Donald Broadbent (1958, 1971) was the first to put this idea forward as the basis for a sophisticated account of the nature and mechanisms of attention in the form of his Filter Theory. In his own words,

> '... selection takes place in order to protect a mechanism of limited capacity...'
> - Broadbent (1971, p. 184)

> 'Large though the brain is, any conceivable mechanism which could cope simultaneously with all possible states of the eye, the ear, and our other receptors, would probably be even larger.'
> - Broadbent (1971, p. 9)

It is interesting to look at the factors which contributed to Broadbent's Filter Theory and his posit-

[1] This is similar to an idea that has been mooted for Cognitive Psychology by Massaro (1987).

ing of a limited processing capacity. Broadbent's theory was based on applied research examining the abilities of air traffic controllers to shadow (listen to and repeat) messages from one ear in the presence of other messages, in particular the patterns of deficits in performance that they showed. This selective listening task became the paradigmatic approach to studying attention (Anderson 1995). So Filter Theory was based predominantly on the results of experiments in auditory, not visual, attention. This is an important point for researchers of visual attention, as there is some debate as to the generalisability of theories of attention from one modality to another (see Neumann, van der Heijden & Allport 1986 for comments on this issue).

Given the applied nature of Broadbent's initial research, it is not surprising that he borrowed heavily from the concepts of Engineering Psychology, in particular Information-Processing Theory[2]. In brief, Information-Processing Theory is a theory of the communication of information. One of its tenets is that when the rate of information transmission through a finite capacity channel exceeds a certain threshold, error is introduced into the information being transmitted. Since the brain is a finite system, Broadbent reasoned, and it transmits information, there must be some limit to its capacity to process information (Neisser 1976). The results of people in selective listening tasks seemed to conform with this view.

The function of attention according to Broadbent was therefore to protect a cognitive system of limited processing capabilities from being overloaded by the vast horde of information received via the sense organs. This seems an entirely reasonable assumption to make. Once this assumption is accepted, the implications are obvious:

> 'If a complete analysis were performed even on neglected messages, there seems no reason for selection at all.'
>
> \- Broadbent (1971, p. 147)

It is impossible to underestimate the impact of Broadbent's Filter Theory on researchers in attention. Although the theory itself is no longer considered tenable, its assumptions on the attributes of the cognitive system and its sparking of the early vs late selection debate are easily detected in nearly all modern accounts attention. Researchers assume that limited processing capacity necessitates attentional selection.

But what happens if the causal status of limited capacity is turned around? What if, instead of being a cause of attentional selection, limited capacity is actually a product of attentional selection? That is, by exercising selective attention, the cognitive system acts *as if* it is a limited capacity system. Limited capacity changes from being an *explanation* for attention, and becomes something which is *explained* by attention selection (Neumann 1987, 1996; van der Heijden 1992).

Perhaps surprisingly, there are good reasons for thinking that this might be the case. Research has yet to show any definite limitations on a human's ability to process information (Neisser 1976; Neumann 1987). Indeed, given the incredible parallelism and modularity of function in the brain, the computational power of the cognitive system is likely to be immense (Neisser 1976; Neumann 1987, van der Heijden 1995). Neumann (1987) gives the control of upright stance as an example of the extent of the brain's ability to integrate and process complex sensory information without any sign of capacity limitation. He notes that in this task,

> '... visual information about contour and motion, vestibular information from the orthostate and the canals, and proprioceptive information from the joints, the muscles, and the skin are all processed in parallel and integrated ....'
>
> - Neumann (1987, p. 362)

From this he concludes that

> '... the brain's capacity is very large compared to the limits of attentional capacity in such apparently simple tasks as dichotic listening ..., or reacting to two consecutive stimuli in the refractory period paradigm ..., or pressing a button in response to a tone while engaged in a simple matching task ...'
>
> - Neumann (1987, p. 362)

This is an important point. If we display such marked limitations in our capacity to perform such relatively simple tasks as outlined in the above quote, how are we able to carry out such computationally challenging tasks as maintaining an upright stance during locomotion effortlessly and without deficit in our everyday actions? If our cognitive system was as limited in capacity as some theorists tend to think, we simply would not be able to function in our complex environment at anywhere near the level that we so plainly do.

In a sense, of course, the cognitive system must be limited in its capacity to process information. As Broadbent proposed, it is a finite physical system after all. The argument here though is that while this statement must be true at a trivial level, we have not established how limited our capacity to process information really is. What we already know suggests that it is quite high, and certainly much higher than evidence from selective attention research would suggest. This in turn implies that limited capacity is not the real reason for the existence of attentional selectivity, it is merely a product of it. At the very least, such considerations clearly indicate that the assumption of limited capacity is not a useful constraint for theories of attention[3].

Attentional Selection: Early, Late or Neither?[4]

At the time that Broadbent's Filter Theory was put forward, the computer metaphor was gaining popularity within what would become Cognitive Psychology[5]. Rapid progress was being made in the development of the digital computer. The possible similarities between the information processing capabilities of these machines and the human cognitive system did not escape workers in Cognition. Early theories of attention

[2] Note that this is distinct from the Information-Processing Approach in Cognitive Psychology.

[3] Some have argued that although not acceptable as an explanation for data obtained in psychological studies of attention, the notion of limited capacity still retains some utility in that it works well as a description of performance in real world situations (eg Moray 1993). While this is a valid point, it highlights the distinction between limited capacity as an explanation of attention, and limited capacity as a product of attention. It this case, limited capacity only fills a role as a descriptive term, not a causal agent. Thus, limited capacity can still fulfil a useful role, but not as a causal construct.

[4] Apologies to Navon (1989) for stealing his title.

[5] Most psychologists consider the publication of Neisser's *Cognitive Psychology* in 1967 as first formal definition of the field. Broadbent's first full formulation of Filter Theory was published in 1958.

therefore took the architecture of the serial digital computer as the framework under which to construct their models. Like the assumption of limited capacity, the use of the computer metaphor has had enormous impact in the field of attention, and has fuelled controversies that are still very much with us today.

One of the key elements of the common digital computer is that it processes information in a serial, stage-by-stage fashion. In other words, when information is presented to a computer via some input device, it goes through discrete stages of processing, one after the other, before gaining access to the Central Processing Unit (CPU). The CPU itself has very definite limits on its information-handling capacity. For example, it can only process one bit of information at a time. Once the CPU has finished with the information, the appropriate action is taken with it. It may be sent to an external device such as a monitor or disk drive, or it may simply be stored. So the information has to pass through quite distinct stages. While a particular stage may process information in parallel internally, the overall flow of information between stages is serial.

Theories of attention are generally based on this serial model of processing. Information passes through a series of stages, starting with an analysis of basic perceptual properties like spatial location, colour and orientation. These attributes are considered to be computationally inexpensive. After this stage, higher level processing such as semantics and categorisation are performed. In contrast to the basic perceptual properties, these attributes are considered to require a marked amount of computation to analyse. Once this stage is completed, any further processing and cognitive manipulation is performed. Finally, the processed information is used in the emission of a response or stored in memory.

While theories of attention do differ significantly from each other in the type of cognitive architecture they put forward, most are in agreement with the idea that information is processed in a predominantly serial manner, with basic perceptual properties being analysed at an early stage and semantic attributes being processed much later.

With a conception of the cognitive system as being essentially serial in nature, a logical question to ask is where in the chain of processing selection takes place. Does attentional selection take place on the basis of basic perceptual information only, and so after the early perceptual analysis stage, or does it occur later on, after a more or less complete analysis of the stimulus has been performed? These two viewpoints, termed Early and Late Selection respectively, are shown graphically in Figure 1 (overleaf). Most theories of visual attention fall into one of these two viewpoints. Hybrids do exist, but these are comparatively rare. The two positions of the locus of attentional selection differ not only in where they posit selection takes place and the attributes it uses, but also on the issue of what form of limited capacity that attention is protecting. Early Selection theories are exemplified by Broadbent's Filter Theory. Selection exists because the cognitive system does not have the capacity to fully process the incoming stimuli. Hence selection occurs early on in the processing sequence so that only the stimuli needed are permitted access to semantic and further processing. By contrast, Late Selection theories place the locus of selection after the incoming stimuli have all been processed fully, or at least to the semantic level. Attention is therefore not serving to protect a limited capacity cognitive system. Rather, attentional selection is necessary because we are limited in our capacity to respond to information. For example, there are quite obvious physiological limita-

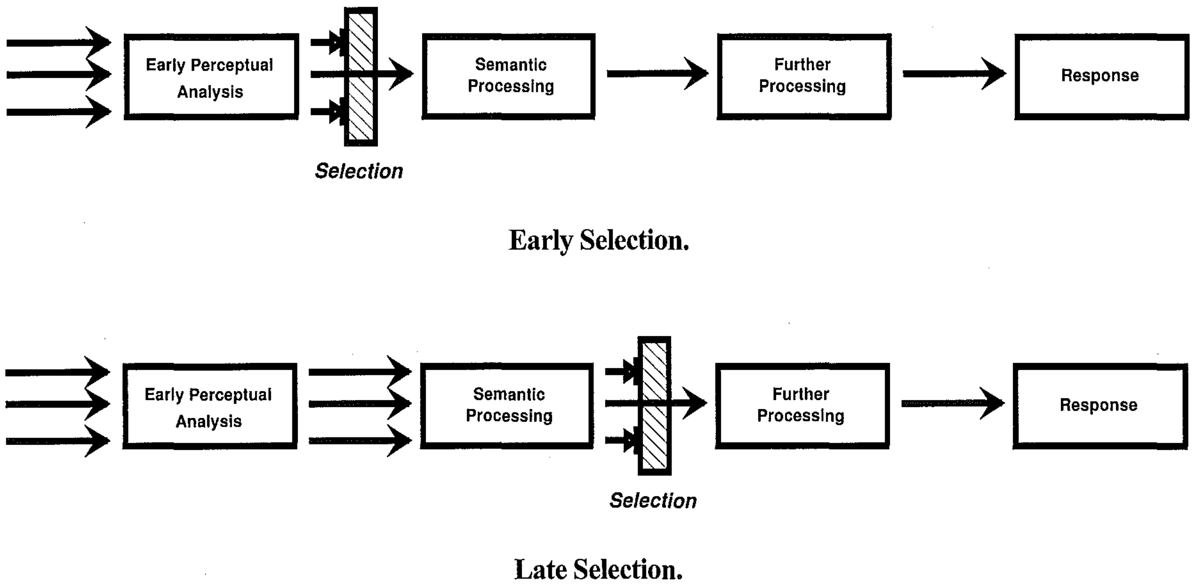

Figure 1: Diagrams representing the flow of information and the point at which selection takes place for Early and Late Selection theories.

tions on the responses we can emit. So just by changing the proposed location of selection, the assumptions underlying the reason for attention change significantly.

There is a large corpus of research into the issue of whether attentional selection is early or late. Indeed, this has been a major focus of inquiry into attention over the past 25 years. Yet the literature is quite inconclusive as to which of the alternatives is most likely to be correct (Allport 1989; 1993). The best conclusion that can be reached is that both viewpoints are partially correct, but neither is adequate to account for the experimental data on its own. Early perceptual features, especially location, are important in selection, and may provide particularly efficient selection criteria (van der Heijden 1992), but attention can be captured relatively easily by 'late' characteristics of the stimulus, such as meaning (Anderson 1995)[6].

It is tempting to view Early Selection as being merely a special instance of Late Selection. In other words, the stimulus is completely processed automatically, but early perceptual properties can still be used as the sole selection criteria. This appears a reasonable compromise to make. However, it still hinges on the assumption of a sequence of serial stages in the processing of perceptual information[7]. It is quite debatable that the cognitive system is actually structured in the manner that this position assumes. Neurophysiological evidence points to the brain being a highly parallel, modular processing system (Allport 1989, 1993; van der Heijden 1995). Furthermore, it appears that the brain contains two semi-independent pathways for processing visual information. One, the 'ventral stream' is implicated in the processing of infor-

[6] A good example from everyday life of attentional capture by so-called late stimulus properties is the Cocktail Party Effect (Cherry 1957). A person engaged in a conversation at a party will be focussing their auditory attention on the speaker's voice, and blocking out the other conversations occurring around them. Yet if someone within earshot happens to mention the listener's name, the listener will most likely begin paying attention to the person that spoke their name. This suggests that even though the surrounding conversations are blocked out, they are still processed to a sufficiently high level for selection to occur on the basis of semantic content.

[7] One can put forward weaker versions of these positions which make no assumptions about the underlying architecture of the cognitive system. This would be to simply argue that selection takes place on the basis of simple perceptual or semantic properties of the stimulus exclusively, without reference to the actual locus of selection. Such a position still suffers however from the preponderance of evidence which supports both positions.

mation about object identification, while the 'dorsal stream' seems to process information important to object manipulation, such as orientation, location and size (Goodale & Milner 1992; Jeannerod 1994; Jeannerod, Arbib, Rizzolatti & Sakata 1995; Milner, Carey & Harvey, 1994; Milner & Goodale 1993). These visual streams can be thought of as conforming to what is meant by early perceptual properties and late semantic attributes. This poses obvious problems for the Early and Late Selection positions. If early and late properties are processed in parallel and largely independently, the debate over the locus of selection as it has been formulated by these positions does not make sense.

Attention As A Unitary Mechanism

> "Everyone knows what attention is."
>
> \- James (1890/1950, p. 403)

Considering the amount of time and effort devoted to studying attention by researchers in Cognitive Psychology, it is rather surprising to learn that there is very little agreement about what attention actually is. One of the few points of agreement appears to be that attention is a unitary, causal mechanism (Johnston and Dark 1986, in Allport 1993). That is, there exists some cognitive process called attention that is a determinant of behaviour. But how realistic is the idea of a unitary attention? It is informative to look at a partial list of phenomena that have been attributed to the effects of attention:

> 'Consider just a few such phenomena: negative priming; induced saliency; visual pattern masking and "demasking"; temporal grouping and phenomenal simultaneity; perceptual "retouch"; illusory conjunctions in immediate postcategorical memory; semantic priming versus expectancy; voluntary, involuntary, and countervoluntary covert spatial orienting; express saccades; perceptual "prior entry"; Stroop-like interference effects; probe RT delays; load-dependency effects; shifting and engagement of selective set — between stimulus dimensions, between cognitive operations; language switching; attentional dyslexia; center-of-gravity effects in saccadic control; "dilution" of the Stroop affect *(sic)*; vigilance decrements; spatial route planning; exogenous and endogenous control of visual search trajectories; inhibition of return; keeping track of current, and completed, goals and subgoals; evaluation of action consequences; covert target detection; endogenous action choice; suppression of prepotent response tendencies; "planning and problem solving"; and so on.'
>
> \- Allport (1993, p. 204)

This is a diverse range of phenomena. It is highly doubtful that any researcher would argue that all of these are caused by a single cognitive mechanism. To do so requires the positing of a mechanism so unspecified that it has the same explanatory power of saying that a particular phenomenon is due to perception or thought (Allport 1993; Neumann 1987). This is no explanation at all.

Attention as a unitary causal mechanism is therefore not a sustainable assumption. If the concept of attention is to be of any use to Cognitive Psychologists, it must be viewed as a label given to mechanisms which realise a common *function*, such as the selection of incoming information for the control of action (Allport 1993; Neumann 1987, 1990; van der Heijden 1992). This point is reinforced when neuropsychological data on the organisation of the visual system is examined in a later section.

<u>*Summary*</u>

The typical approach to the study of visual attention has involved the use of highly simplistic experimental analogues of real-world situations. However, this approach has not led to any major progress in solving some of the fundamental issues in the study of attention since modern research in the area began. A satisfactory definition of attention has yet to be found, and researchers are still unsure of where in the stream of processing selection actually occurs. As was argued above, this may be due to the assumptions underlying research into visual attention being wrong. The next section deals with an alternative approach to studying visual attention which provides a quite different view of perception, and the way in which it should be studied.

One Alternative: An Action-Oriented Approach

If the currently dominant method of studying visual attention is no longer appropriate, as maintained above, what alternatives are available? In this section, an action-oriented approach is put forward as a step towards greater ecological validity in attentional research and, as such, a compromise between the extremes of artificial yet tightly controlled experimental analogues on the one hand, and real-world settings but less stringent experimental control on the other.

The theoretical underpinnings of an action-oriented approach derive mainly from Gibson's (1979) Ecological Psychology and the recent upsurge of interest in evolutionary ideas in Cognitive Psychology. The main contention here is that the logical separation of perceptual from motor processes assumed in most psychological research is invalid. The reasoning for this contention is in many respects elegantly simple. What is the function of perception? What benefits did it confer to our ancestors that caused the evolution of the sophisticated perceptual machinery that we now possess? Obviously, perception evolved because it provided information about the environment, both external and internal, which allowed actions to be successfully performed. The benefit of this to the individual's reproductive fitness is immense, but clearly only if the individual has the ability to act on the information provided. Therefore, for perceptual ability to have been of any evolutionary advantage, it had to provide information that the motor processes both needed and could use. This suggests a tight integration of perception and action as they co-evolved (Bootsma 1989; Braddon 1986; Heft 1993; Michaels & Carello 1981; von Hofsten 1985, 1987, 1990). It also implies that the function of attention might be to aid in the process of making sure that the motor systems were provided with perceptual information relevant to the current behavioural goal.

So perception's function is to provide the information required to the motor systems. The information required is of course action-dependent and, by extension, goal-dependent. However, this summation does not do justice to the interplay between what is termed perception and what is termed action. As Gibson (1979) and Micheals & Carello (1981) have rightly pointed out, much of an individual's behaviour can be classified as exploratory in nature. In other words, organisms have a tendency to actively seek out the information from their environments. In this respect action dictates what is perceived. So perception really should be characterised as a 'perception-action cycle'[8], where perception informs action, and action guides what can be perceived.

An intriguing possibility arises here when one considers the course of the co-evolution of perception and action. The evolution of any characteristic, morphological or psychological, follows a gradual progression from simple to complex (Dawkins, 1986). Just as the eye of a mammal is much more likely to have evolved from a single photoreceptive cell rather than sprung forth in its present form, so too with perception and action. Furthermore, evolution tends to build in a modular fashion, using basic building blocks and adding to and combining them in novel ways. The remarkable perceptual-motor processes possessed by humans almost certainly began as something as basic as a simple reflex arc. The behavioural requirements for survival in the environment would obviously have been the key selective pressures in the evolution of the perception and action systems. So tightly integrated perceptual-action systems for basic behaviours, such as reaching, grasping, throwing and dodging, would have been likely to evolve. In essence, specialised perceptual-action systems would have developed to deal with the fundamental behavioural requirements imposed by the environment (Allport 1980, 1987, 1989, 1993; Bootsma 1989; Gibson 1979; Heft 1993; Neumann 1990; Neumann, van der Heijden & Allport 1986; Pratt & Abrams 1994; Tipper, Brehaut & Driver 1990; Tipper, Howard & Jackson in press; Tipper, Lortie & Baylis 1992; van der Heijden 1992, 1996; von Hofsten 1985, 1987, 1990; Warren 1984).

Clearly, this view presents a vastly different conception of perception and action to that prevalent in Cognitive Psychology. Not only are perception and action seen as logically independent in most research, but they are also considered to be, in Cosmides & Tooby's (1987) terms, 'general purpose' information processing modules. In other words, they can process a wide range of information; they are reasonably unspecialised. By contrast, an action-oriented approach sees perception and action not only as part of the same whole, but also as made up largely of specialised, 'domain-specific' processing modules. Thus, the perceptual-action system is comprised of processing modules which have evolved to deal with specific environmental information. General purpose processing modules are an exception, not a rule.

These observations on the nature of perception and action translate into a particular approach to visual attention research. The highly abstracted tasks used in current research, while having produced valuable knowledge, can only go so far. Because they look at only one aspect of the perception-action cycle, interesting phenomena which occur in natural movements are effectively masked from investigation. Hence the action-oriented approach aims to employ more ecologically valid tasks in its research, whilst still maintaining experimental control and rigour.

The key point here is that an action-oriented approach does not mean a fundamental change in the way cognitive psychologists undertake experimentation. In this way, an action-oriented approach represents a compromise between naturalistic, uncontrolled methods and the tightly constrained artificial laboratory studies. It seeks to combine a greater degree of ecological validity into experimentation through the use of more natural tasks which preserve the perception-action cycle whilst still employing the methods and goals of Cognitive Psychology. By doing this, phenomena of visual attention which had so far remained

[8] This is Neisser's (1976) phrase.

undetected or intractable become amenable to scientific examination.

Thus far the way in which an action-oriented approach represents a change from the current norm in visual attention research in two of the three stages of research has been examined. First, it presents a markedly different conception of visual attention with which to formulate hypotheses and frame theories. Second, it argues for the use of more natural tasks and settings in experimentation. And finally, it places definite limits on the scope, or generalisability, of results from attention studies. The assumption that the cognitive system is essentially a general purpose processor has allowed researchers to generalise the results of their experiments to attention as a whole. The classic example of this is the use of data from experiments on auditory attention by Broadbent (1958) and Treisman (1969) to attention in general. In the case of vision at least, there are good arguments against this sort of cross-modality generalisation (see Neumann, van der Heijden & Allport 1986). But if the perception-action system is composed primarily of domain-specific subsystems, as the action-oriented approach contends, then the generalisability of results have to be demonstrated, not simply assumed. What is found to be true for one particular subsystem may not be true of others.

The action-oriented approach therefore offers what is at once quite a new yet reassuringly familiar way for studying visual attention. It involves not so much a change in method as a change in perspective and conceptualisation. Some of these changes are already apparent in the research literature in the work of cognitive psychologists that subscribe to a selection-for-action view of attention, such as the work of Steven Tipper and his colleagues[9]. The next section deals with this and other research which suggests that perceptual stimuli are processed quite differently depending on the intended action response.

Empirical Evidence for the Relevance of Action

The preceding section argued that an action-oriented approach to visual attention, and perception in general, would overcome some of the deficiencies in the approach which seems to currently enjoy popularity. One of the central tenets in an action-oriented approach is that perception and action cannot be viewed separately, that each is part of the same cycle and influences the other. This is an empirical question. Evidence obtained in behavioural and neuropsychological studies do point to close links between perception and action systems. This section summarises some of the research which supports this view of perception and action.

In a study examining the use of the invariant *tau*[10] for timing the hitting of a falling ball, Bootsma (1989) neatly demonstrated how the type of response can influence overall task performance. Using the same stimulus in each condition, Bootsma divided his participants into three different response categories. One group was required to actually hit the falling ball with a bat. The next group had to press a button which caused an artificial arm to swing at the falling ball. The final group simply had to press a button to

[9] E.g. Tipper, Brehaut & Driver (1990), Tipper, Howard & Jackson (in press), Tipper, Lortie & Baylis (1992), Tipper, Weaver & Houghton (1994) and Tipper, Weaver, Jerreat & Burak (1994).

[10] Tau is the relative rate of expansion of an object as it approaches the perceiver, and has been shown to be used by the perceptual

indicate at what point in its fall the ball should have been hit. Bootsma compared these groups on the number of hits they achieved or would have achieved and the initiation time of their response.

Bootsma's results lend credence to the notion that the response required greatly affects performance. Participants who actually hit the ball with a bat scored more hits and showed significantly less variation in the initiation time of their swing than the other two groups. The participants who pressed a button to swing an artificial arm scored the next highest, and had increased variation in their initiation times. The group who simply had to press a button performed worst overall. Thus, despite having identical visual information specifying the appropriate time to strike, performance varied according to the type of response required.

Not only do Bootsma's results suggest that varying the mode of response greatly influences performance, even when perceptual stimuli remain constant, but they also provide support for the hypothesis that the perception-action system is primarily composed of specific subsystems which have evolved to deal with common perceptual-motor tasks. As the mode of response progressed from the naturalistic striking a moving target with a hand-held object to the quite abstract task of analytically estimating the appropriate time to strike without performing the action, performance degraded. In Bootsma's (1989, p. 492) own words,

> 'If perception is for doing, as Gibson (1979) has it, then the more "what has to be done" is separated from the situations in which perception-action systems evolved, the more inaccurate perceptual processes subserving the required perception-action coupling are likely to be.'

Bootsma (1989) is not the only researcher to obtain results indicating the presence of specialised perceptual-action subsystems. In an ingenious study, Heft (1993) tested the hypothesis that the more analytical a perceptual judgement is, and hence the less like the evolved perceptual-action coupling, the less accurate performance employing that judgement will be. Again participants were divided into three groups corresponding to the tasks they would be required to perform. The first group was required to verbally estimate whether a pen placed on a table in front of them was within their reach. No time limit was placed on their response. The second group also performed this task, but with a two second time limit to respond imposed on them. The final group were required to complete a puzzle which necessitated the use of a pen. The pen was placed on a table as for the other groups. Unlike the other groups, however, this group were told to reach and grasp the pen if it was within reach. If it was not, they simply had to leave it there and continue with the puzzle. The actual reaching for the pen was therefore a subgoal of the overall task, and not the focus of it. In this way Heft hoped that the distance estimation of the third group would be largely implicit.

The results obtained by Heft (1993) support his contention that the more analytical a perceptual judgement is, the less accurate it will be. The group which performed the reaching movement as a subpart of another task were significantly more accurate in their estimations of whether the pen was within reach than the other two groups. For these two verbal report groups, the amount of time available to make the judgement did affect accuracy, with the group allowed only two seconds in which to make their judgement

system to calculate the time to arrival of an object (Lee 1976).

performing slightly better than the group with no time limit.

These results are in line with those gained by Bootsma (1989). By making the judgements more analytical, Heft (1993) was effectively moving the task further away from the perceptual-action coupling context in which it had evolved. These results are what the action-oriented approach would predict, and provide a warning about generalising the results of abstract laboratory experiments in visual attention to everyday performance.

This distinction between analytical and action modes of perception has support from neuroscientific data. Ungerleider and Mishkin (1982) brought to widespread attention that the primate visual system seems to contain two semi-autonomous streams for processing visual information required for different purposes. The dorsal stream appears to process information critical to spatial vision and visuo-motor coordination, and is often characterised as the 'where' channel. The ventral stream, on the other hand, appears important for form-based object recognition, and is commonly referred to as the 'what' channel (Jeannerod 1994; Maunsell & Ferrera 1995; Milner, Carey & Harvey, 1994; Posner, Grossenbacher & Compton 1994; Vaina 1990). These two pathways are largely both anatomically and functionally distinct.

While the separation of visual processing into what and where streams has proved useful, and has unified a large body of research data, it has recently been proposed that while there are indeed two semi-autonomous visual pathways, classifying them as processing what and where information may be a misleading oversimplification. Based on their evaluation of the neuropsychology literature, Goodale & Milner (1992; Milner et al. 1994; Milner & Goodale 1993) have argued that the two visual streams may better be characterised as having the functions of providing information for the on-line control of action and for off-line high-level perception[11] respectively. In this way, the difference in function is not 'what' versus 'where', but 'what' versus 'how'.

Goodale & Milner provide support for their views by re-examining the neuropsychological data that has generally been thought to validate Ungerleider & Mishkin's (1982) distinction between 'what' and 'where' processing streams in humans. Tests performed on patients suffering from what is known as optic ataxia have shown that these people can easily recognise objects presented to them, but have considerable trouble accurately reaching for them. Optic ataxia occurs when damage is sustained in the areas of the brain through which the dorsal pathway runs. Conversely, patients suffering from visual agnosia are unable to recognise or even describe items presented to them, but can easily navigate their way through their local surroundings. Visual agnosia is associated with damage to the areas of the brain which contain the ventral stream. Clearly, these results provide strong support for the existence of a 'what' and a 'where' channel in visual processing.

Further analysis of the symptoms presented by patients with optic ataxia and related disorders[12] suggests that more than just the patients' ability to appropriately process information about spatial layout is impaired. Patients with this disorder not only showed markedly diminished ability to move their hand in

[11] Such as visual learning, planning and object recognition. Milner & Goodale extend this to conscious perception as a whole, but it is not necessary to take this step for the argument being developed here.

[12] Such as Balint's syndrome and other disorders characterised by damage to regions of the brain thought to contain the dorsal pathway.

the right direction when reaching for an object, but also in positioning their hand to the orientation of the object or scaling their grip correctly. So not only is spatial layout ability impaired in optic ataxia, but also the ability to use information about the size, shape and orientation of an object in visually-guided actions (Goodale & Milner 1992; Milner & Goodale 1993).

Likewise, a closer look at the symptoms of patients suffering from visual agnosia reveals the sparing of abilities that Ungerleider & Mishkin postulated would have been impaired. Visual agnosiacs are unable to recognise objects or describe properties of them, such as shape, size and orientation. This led Ungerleider & Mishkin to conclude that object-based information should not be available for any purpose when the ventral stream has been damaged. But while patients with this form of brain damage can not report properties of an object when asked, they have few problems actually manipulating the object, which implicitly involves the use of information about the object's properties. The following description of a patient suffering from visual agnosia by Milner & Goodale (1993, pp. 331-32) demonstrates this clearly:

> 'Thus, when presented with a large slot which could be placed in one of a number of different orientations, [the patient] showed great difficulty in indicating the orientation of the slot either verbally or even manually by rotating a hand-held card. In contrast, simply to reach out and post the card, [the patient] performed as well as normal subjects, rotating [their] hand in the appropriate direction as soon as [they] began the movement ... [the patient's] performance was indistinguishable from the two controls.'

The neuropsychological evidence points towards the existence of two relatively autonomous pathways for processing visual information. One appears to specialise primarily in processing information needed for the on-line control of action, the other in processing information used in the rather more indirect control of action, where a more analytical and reflective mode of cognition is required. This fits nicely with the dichotomy proposed by Heft (1993) between a perceptual and an analytical mode of cognition. Furthermore, it provides converging evidence for Bootsma's (1989) inferences regarding the existence of specialised perception-action systems for basic perceptual-motor tasks. Additional neuropsychological evidence showing that the motor and visual cortex areas are extensively and quite directly interconnected lends further credence to this claim (Allport 1987, 1989, 1993).

Taken together, the behavioural and neuropsychological evidence provide a persuasive argument for the strong link between perception and action. The mode of response has been demonstrated to impact on task performance, yet Cognitive Psychologists ignore this. Perceptual phenomena can not be studied in isolation from action. The two are not logically independent, as has simply been assumed. Future research must take this into account.

Furthermore, the data from these neuropsychological studies suggest that phenomena commonly ascribed as being due to a mechanism called attention are realised in functionally distinct anatomical areas. This reinforces the argument that attention can only really seen as a functional label rather than a unitary causal mechanism.

Summary

The above considerations call into question the appropriateness of the dominant methodology for studying perceptual phenomena in Cognitive Psychology. The use of highly unnatural experimental situations which appear analogous to real-world situations at an abstract level, while certainly providing valuable data and useful insights, can only take us so far in our search for a full understanding of perceptual phenomena. Certainly, they give us some idea of how the perceptual and cognitive systems function when presented with the quite abstract tasks required by modern life. But they tell us very little at the level of basic interaction with the world. As the evidence cited above suggests, the performance of the tasks which people have had to carry out over the millennia simply to exist, such as navigating through their environments and interacting with objects, requires the use of quite distinct perceptual subsystems that evolved specifically to deal with these tasks. It can not therefore automatically be expected that the results of research using artificial environments will generalise to performance in these basic tasks. Indeed, the evidence strongly suggests that the results will not generalise, in either direction.

This comprises the rationale for the experiments presented in the next section. These two experiments are intended to demonstrate that using a task which preserves the perception-action cycle by employing a basic perceptual-motor task can lead to very different results to those obtained using less natural activities. In this way, the danger of generalising from research using these less natural techniques to the real-world will become apparent. It is also hoped to show that by keeping the perception-action cycle intact, lines of research which would have remained invisible can be discovered.

Experiment One

The intention of this experiment was to replicate the salient features of the design and results of an existing experiment on selective attention using active object manipulation. The experiment chosen for this purpose was Experiment 1 of Tipper, Lortie and Baylis's (1992) study of action-centred representations and attention. Tipper et al. (1992) used a 3x3 grid of buttons and associated target and distractor lights. Participants were required to respond to the presence of a red target light by pressing the button directly behind it. In addition, some trials contained a yellow distractor light which was to be ignored. The results obtained conformed to those of previous research, showing the characteristic degradation in response time[1] when a distractor was present. The truly interesting finding that Tipper and his colleagues uncovered was that distractor interference effects were significantly larger when the distractor lay on the movement path of the hand as it reached towards the target. This effect appeared regardless of whether the participant reached away from their body or towards it. Because the critical factor in determining interference here was whether the distractor lay on the movement path, and not the location of the distractor in relation to the participants' main torso, alternative explanations suggesting that the participants used environment- or retinotopic-centred frames of reference to encode the task were ruled out. Physical obstruction by the distractor object, leading to the modification of the movement path during reaching to avoid it, also could not present a rival explanation, since the distractor and target lights and buttons were not high enough to cause obstruction of any movements.

These findings suggest that people do not just use representations of the target object and its location in reaching movements, but that they also represent the path of movement from the effector starting position to the target location along with any possible competitors for response which lay on that path. These Tipper et al. (1992) term *action-centred* representations.

The beauty of Tipper et al.'s (1992) study was that by involving a more natural action component to the task, it enhanced the ecological validity of previous findings on selective visual attention. Whereas the vast bulk of research on attention has taken place using stimuli and response pairings that do not have great generalisability to everyday human life, Tipper et al.'s (1992) study did. It is not difficult to see the situations in which people reach and interact with a target object in the presence of other distractor objects with similar functions. We do it every time we pick up one glass from a table full of them at a bar, for example.

This experiment represents an attempt to extend the generalisability of the results of Tipper et al.'s (1992) study by replicating Experiment One using an extension of methodology. Essentially, exactly the same procedure is employed, except that the participants are required to pick up actual objects rather than simply press a button. This involves a further material deviation from the logic of Tipper et al.'s (1992) design, in that the stimulus object is also the target for response. In Tipper et al.'s (1992) design, the stimulus

[1] Although some researchers use Response Time and Reaction Time interchangeably, this is not done here. Response Time as used here and subsequently is defined as the time taken from the initial presentation of a stimulus through to the completion of the response required, or when referring to an unspecified time measure. Reaction Time, on the other hand, is defined as the time taken

and target were closely located spatially, but in terms of response, completely dissociated[2]. In this sense, then, this replication was not just a methodological extension, but also an attempt to broaden the scope of previous findings.

Another material difference between Tipper et al.'s (1992) study and the present one is that in the present experiment response time is divided into its component parts of reaction time (RT) and movement time (MT). As this was not done in the original study, it was impossible to tell precisely where distractor interference took place in the chain of processing. If interference occurred during the planning phase, then interference should manifest itself in longer RTs. On the other hand, interference in the kinematics of the movement should be present in the MT.

Previous research examining this area has produced mixed findings. Castiello (1996) tested this issue using reaching for fruit as the motor task. He found no effect on RT, but significant interference in MT. However, his results are difficult to interpret due to confounds in his design. The target fruit was always located in the same position in front of the participant, essentially allowing them to perform any preparatory selection processing in advance of the actual trial. Furthermore, the method of restoring vision to the participant at the beginning of a trial (moving a mask) was such that it would allow the participant a small window of time in which to appraise the visual field. Taken together, it is not surprising that the only significant interference effect he found was present in MT, and that this only applied when the distractor was larger than the target (for example, when an apple was the distractor and a cherry was the target).

Another study attempting to determine the location of interference was carried out by Pratt & Abrams (1994). Their results showed interference effects in both the RT and MT. It seems from these studies that whether the distractor interference manifests itself in the RT or MT depends on two factors. Firstly, if some physical obstacle has to be avoided in the reach for the target object (such as a large distractor in the presence of a smaller target) in a percentage of the study's trials, then this will be reflected in the variation of MTs. Where distractor and target size are held in a constant relation, this should not cause variation. Secondly, the response strategy of the participant may effectively mask the effects of any interference on RT and exacerbate its influence on MT. If a participant adopts a conservative strategy by maintaining pressure on the start key until they have formed an action representation for the task, then one would expect RTs to reflect interference. But if the participant was less conservative in their response strategy, they may release pressure on the start key and then pause while they form an action representation before completing the task. RTs would then be quite uniform, with any variation due to distractor effects being subsumed within the MT.

This is a significant methodological point: If the intention is to determine where the locus of interference is, assuming it is not actually manifest in both RT and MT anyway, we have to somehow control for the participant's response strategy. One possibility is through the appropriate instructing of the participant. Emphasising accuracy may lead to the adoption of a more conservative strategy, while emphasising

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between the onset of a stimulus and the initiation of a response — the time taken to show a physical reaction to a stimulus.

[2] Tipper et al. (1992) refer to their reaching and pressing task as analogous to a situation in which the stimulus indicating target location and the actual target are the same. If the coloured lights Tipper et al. (1992) used in their experiments were also buttons to be responded to, then the analogy would hold. However, it can be plainly seen that it does not.

speed may have the reverse effect. This does not seem like a particularly reliable method, however, and it would be difficult to show that any effect on RT was due to an effect rather than just the instructions themselves. A more tenable method would be to track the velocity of the initial ballistic movement, so that the cut off point between RT and MT resides at a particular velocity. By use of the appropriate cut off velocity, the MT phase will not start until the participant is performing the rapid movement which characterises the initial phase of the reach-to-grasp movement (Jeannerod 1988). This method is commonly used in kinematic studies of limb movement (e.g. Chieffi, Gentilucci, Allport, Sasso & Rizzolatti 1993; Stins & Michaels in press; Tipper, Howard & Jackson in press). However, lack of the appropriate resources prevents this approach from being adopted in the present experiment.

In summary, this first experiment aims to reproduce the findings of Tipper et al. (1992) in a more realistic reaching situation. It is therefore predicted that distractors in front of the target, and so on the movement path, will produce more interference than distractors which are not on the movement path. Furthermore, distractors on the same row as the target are predicted to cause more interference than those behind, but less than those in front of, the target, since they will be more likely to fall on the movement path than distractors placed behind the target. It is also expected that the fastest condition will be when no distractor was present at all. Given the discussion above, no predictions can be made about which of the time measures will be influenced the most by the target-distractor coupling. By breaking the total response time into its component reaction and movement times, however, effects in one of these time measures which may be obscured in the total time by the variations in the other time measure should be apparent. Therefore, for any of the predictions made to be validated they must be upheld by one, but not necessarily all, of the time measures.

Method

Participants

Sixteen student volunteers (11 females) took part in this study. Ages ranged from 18 to 44 years (mean: 26 years). All participants were right-handed and had normal or corrected-to-normal vision according to self report. No participants reported any deficiencies in colour perception. All of the participants were able to comfortably reach to the fourth row on the stimulus board. A small cash payment of $8.00 was made upon completion of the experiment.

Apparatus

The apparatus used in these experiments consisted of a stimulus board, target and distractor objects, and a start button. The stimulus board was a 500 mm x 550 mm board painted black to minimise reflectivity.

Locations where objects were placed formed a 4 x 4 grid, with the centres of each location being 100 mm distant from each other. A 40 mm square patch of unpainted area was centred on these locations to aid in the accurate placement of objects by the experimenter. Each object location contained two copper strips which, when closed by a copper sheet attached to the bottom of an object, allowed a computer to sense whether an object was present at a particular location. The front row of the location grid was set 130 mm in from the front of the stimulus board. Depending on whether the whole 4 x 4 grid was to be used, or merely the foremost left 3 x 3 subset, the start button was lined up exactly centred to the grid, 50 mm from the front edge of the stimulus board. In the present study, only the foremost left 3 x 3 subset of locations was used.

Since it was essential that the participants be unable to view the experimenter placing the objects on the stimulus board, the board was enclosed in a wooden box 520 mm wide by 550 mm high and 550 mm deep. The front of the box contained two openings. The first was a gap 230 mm high by 500 mm wide, which left a utilisable space of 160 mm between the top of the gap and the stimulus board for the participant to gain access to the inside of the box with their hand. This gap was covered with thick black cloth cut into strips to prevent ambient light from entering the box without hindering movement or the visual perception of hand movements made during the experiment. The second opening in the box measured 405 mm wide by 150 mm high and contained a pane of one-way mirrored glass. When no light was present inside the box, the glass efficiently prevented the participant from seeing the objects within the box. However, when light of a sufficient intensity was introduced inside the box, the glass allowed an unobstructed view of the interior of the box. A flap of thick black cloth was attached to the top of the box and could be draped across the glass in order to act as a further safeguard against the participant gaining any visual cues about the possible location of any objects within the box between trials. A piece of wooden dowelling was attached to the bottom of the material so that it could be easily manipulated by the participant.

The entire back section of the box was open to allow to experimenter access between trials. This too was covered with thick black cloth to prevent ambient light from entering the box. Light was provided during the trials by a 100-watt tungsten filament bulb affixed at the top centre front of the box. This gave good even glare-free illumination. The participants were unable to view the light source directly.

The stimulus board and the lightbulb were controlled by a Pentium PC running at 100 MHz (66 MHz bus speed) via an Intel 8255 I/O card. The PC controlled the switching of the light source and read the stimulus board. It also performed and recorded all time measurements and provided real-time feedback to the experimenter and participant if an error occurred. A set of enclosure headphones attached to the computer's Soundblaster compatible sound card provided an auditory cue for the trial onset as well as feedback if the participant made an error, and effectively impeded the participants' ability to localise any sounds made by the experimenter when placing the cubes between trials by obstructing their pinnae.

In this initial experiment, the target and distractor objects were simply wooden cubes. The target cube was painted red and was 400 mm cubed in size, while the distractor cube was half the height of the target cube and painted green. Both had sheets of copper attached to the bottom of them so that the placement of a cube at a location on the stimulus board closed a circuit with the copper wires on the board. This allowed the computer to detect whether the cubes were properly placed at the beginning of a trial and when they had been picked up at the end.

Design

A one-factor within subjects design was used for this experiment, with the relative position of the distractor cube to the target being the manipulated variable. The measures of interest were the participants' time to react to the displaying of the experimental objects (RT) and the time they took to move and pick up the target object (MT) from the 3 x 3 grid of locations.

There were four ways in which the distractor could be arranged relative to the target. Firstly, the distractor cube could appear on the same row (i.e. horizontal line) as the target. This is referred to as the SR condition. The distractor could also appear on a row in front of the target (i.e. between the participant and the target). This is labelled the FR condition. Conversely, the distractor could appear in the row behind the target (i.e. the target was between the participant and the distractor). This is termed the BR condition. Finally, the distractor could be absent. This is referred to as the ND condition, and was intended to serve as a baseline measure with which interference caused by the other conditions might be measured. In summary, then, there were four experimental conditions which represented possible distractor locations relative to the target object. In three of these conditions the distractor was present, while in the ND condition it was absent.

Figure 2 (page 24) shows the layout of the possible target and distractor locations on the stimulus board. Only trials where the target appeared on the centre row (locations 4 to 6) were used for analysis. The remainder of the trials were designated as fillers, and were not analysed. For each of the centre row locations, the target appeared a total of 24 times. Of these 24, the SR and ND conditions accounted for 4 each, while the BR and FR conditions each accounted for 8. Thus there were a total of 12 trials (4 trials by 3 locations) that appeared in the centre row each for the ND and SR conditions and 24 each for the BR and FR conditions. This means that a total of 72 trials were analysed for each participant.

The positioning of the distractor in relation to the target was restricted by the constraint that for the BR and FR conditions, the distractor appeared at a location adjacent to the target. This constraint did not apply to the SR condition, where the target and distractor could appear on the opposite sides of the stimulus board. The possible pairings of target and distractor locations for each condition are shown in Table 1 (page 24), with the number of those trials appearing in brackets underneath. The first number refers to the target location, the second to that of the distractor.

Overall, there were 134 trials, with the target appearing on the centre row in 72 of them. The remaining 62 trials in which the target was not located on the centre row of the stimulus board followed approximately the same ratio of conditions as the centre row trials. There were 12 trials each of the filler ND and SR conditions, and 19 each of the BR and FR conditions. Table 2 (page 25) details the breakdown of target- distractor pairings for the filler trials following the same format as Table 1. Note that the filler BR and FR conditions can only occur on the front and back rows respectively, while the filler SR condition could appear on either of these rows.

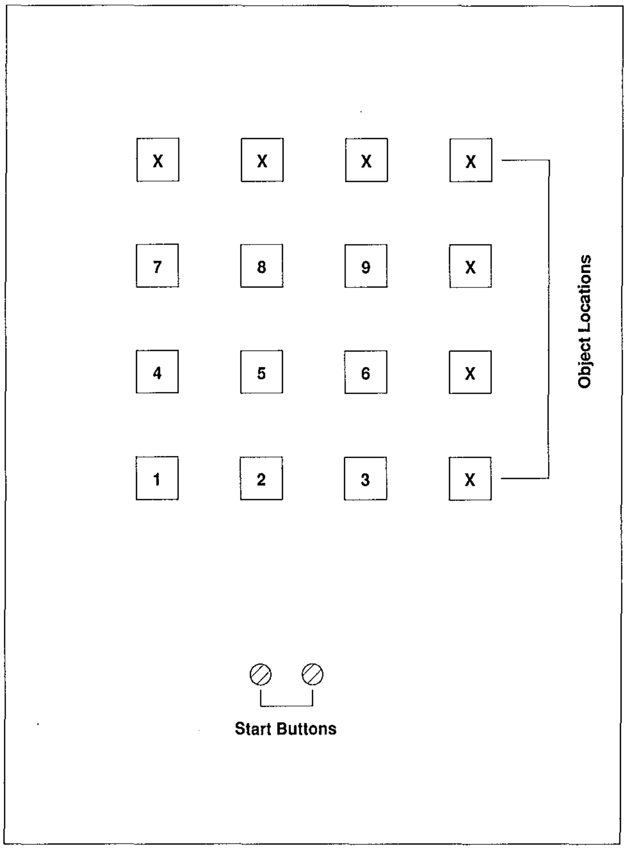


Figure 2: The logical layout of the stimulus board in the first experiment. "X" denotes that that location was not used. Drawing is only approximately to scale.

Table 1: The possible pairings between target and distractor when the target appeared on the centre row for the distractor-present conditions. The numbers in parentheses show the number of repetitions of the trial.

SR	4 - 5 (2)	4 - 6 (2)	5 - 4 (2)	5 - 6 (2)	6 - 4 (2)	6 - 5 (2)	
BR	4 - 7 (4)	4 - 8 (4)	5 - 7 (2)	5 - 8 (4)	5 - 9 (2)	6 - 8 (4)	6 - 9 (4)
FR	4 - 1 (4)	4 - 2 (4)	5 - 1 (2)	5 - 2 (4)	5 - 3 (2)	6 - 2 (4)	6 - 3 (4)

Table 2: The possible pairings between target and distractor in filler trials for the distractor-present conditions. The numbers in parentheses show the number of repetitions of the trial.

SR (front row)	1 - 2 (2)	1 - 3 (2)	2 - 1 (2)	2 - 3 (2)	3 - 1 (2)	3 - 2 (2)	
SR (back row)	7 - 8 (2)	7 - 9 (2)	8 - 7 (2)	8 - 9 (2)	9 -7 (2)	9 - 8 (2)	
BR	1 - 4 (3)	1 - 5 (3)	2 - 4 (2)	2 - 5 (3)	2 - 6 (2)	3 - 5 (3)	3 - 6 (3)
FR	7 - 4 (3)	7 - 5 (3)	8 - 4 (2)	8 - 5 (3)	8 - 6 (2)	9 - 5 (3)	9 - 6 (3)

Procedure

At the commencement of the experiment the participants were shown the equipment used and informed of its function if they were curious. They were then given the target and distractor cubes to hold, and informed that their task would be to reach out and pick up the red cube as fast as they could without sacrificing accuracy by fumbling or sliding the target cube, or dislodging the distractor cube. They were made aware that these would be classified as errors, and that a measure of accuracy as well as speed would be kept. In addition, they were told that releasing the start button before the light inside the box had come on was also classified as an error. Once this was made clear, they were instructed on each aspect of the experimental procedure. Any questions which arose were dealt with.

The participants were seated on an adjustable, non-movable chair facing the front of the box containing the stimulus board. The distance between the participant's torso and the box was adjusted so that the participant could comfortably reach the fourth row on the stimulus board without obstruction of their reach by the front surface of the box. The participant then rested their forehead on the face of the box above the glass window. This allowed some support whilst not providing any significant hindrance to head movements[3].

Throughout the experiment the room was kept in darkness, with the only sources of light being the bulb inside the box, a lamp used by the experimenter to position the objects in between trials, and light emitted by the computer monitor. Both the lamp and the monitor were positioned behind the box so that they were out of the participant's direct line of sight. The ambient light present between trials was therefore minimal.

A piece of dark cloth was draped over the mirrored glass window to prevent the participant seeing inside the box. The experimenter then placed the objects in the appropriate locations for that trial according to information which was displayed on the computer screen. The computer also monitored the stimulus board so that the trial could not proceed until the object(s) were in the correct location(s). At the beginning of a trial the participant removed the obstructing cloth with their left hand. The participant then placed their

[3] Evidence suggests that pointing accuracy can be impaired by restricting head movements with even a chin rest (Biguer, Jeannerod & Prablanc 1985). Also, Tipper et al. (1992) did not restrict head movements in their study. Head movements were therefore not unduly constrained in this study. The participant still retained a reasonable degree of cranial mobility.

forehead against the box surface and peered through the window at where they remembered location 5 (the middle position on the centre row) to be. Note that there was no light present inside the box, so the participant was effectively unable to see through the mirrored glass and obtain any visual information on the arrangement of the cubes.

Once the objects had been arranged and the participant was in position, they placed their right hand on the start button and pressed it. This caused a tone to be sounded in the headphones worn by the participant, indicating that the trial was about to start. A random interval between 500 and 1000 milliseconds elapsed after the start tone to guard against anticipations. The bulb within the box was then lit, providing the participant with the ability to see through the glass window and view the stimulus board and objects. The participant reached out with their right hand and uplifted the target object. The computer recorded the reaction and movement time intervals. If an error was made by the participant a lower pitched tone than that used as a starting cue was sounded through the headphones, thereby providing the participant with performance feedback. Any errors made were also recorded by the computer. The participant placed the cube in the black space between object locations and withdrew their arm from the box. The obstructing cloth was then draped back over the mirrored glass window ready for the next trial by the subject, using their left hand, and the procedure repeated.

Ten practice trials were run at the start of the experiment to familiarise the participants with the task required of them and to make any necessary adjustments to the apparatus. Any queries they had were answered. During the practice trials it was emphasised that participants remember where the centre square (location 5) on the stimulus board was, since they were required to fixate the remembered this location to be at the start of each trial.

Results

Of the 16 participants who took part in the experiment, 2 were excluded. One was excluded due to complications during the running of the experiment which rendered their data invalid, while the other realised the experimental hypothesis. This left 14 valid data sets (11 female). Only the trials in which the target appeared on the centre row were analysed; all other trials were discarded. Likewise, any trials with errors were not analysed further.

The probability of making an error on any particular trial for each condition are shown in Table 3 (overleaf). When compared with the relative times shown in Figure 2 (page 27), no systematic relationship is evident. This suggests that an explanation in terms of a speed-accuracy trade-off is untenable.

Table 3: Table indicating the probability of making an error on a trial for each trial.

Condition	P(Error)
No Distractor	0.018
Same Row	0.036
Back Row	0.012
Front Row	0.033

Separate One-Way Repeated Measures ANOVAs were performed on the median Total, Reaction and Movement time data. The mean medians across conditions and the results of the ANOVAs for each time measure are shown in Table 4.

None of the ANOVAs suggested significance at the 0.05 alpha level, although all of the time measures showed significance at an alpha level of 0.10. The results of a post hoc power analysis reveal that the probability of correctly rejecting the null hypothesis at the 0.05 level with medium effect sizes (Lambda = 2.1) present was only 0.185. This makes the lack of significant findings at the 0.05 level understandable.

The trends apparent in the graphs presented in Figure 2 (page 29) broadly replicate Tipper et al.'s (1992) findings with two notable exceptions. Participants performed faster than expected in the SR condition, where it was predicted that the time measures would be slower than those obtained for the BR condition. Most striking, however, are the slow Reaction and Total Time for the ND condition, which are in marked contradiction to the results obtained by Tipper et al. (1992).

A number of specific directional predictions were derived from Tipper et al.'s (1992) results. These are shown in Table 5 (overleaf). The number of participants out of 14 with results conforming to the prediction was counted and tested against the null hypothesis that an equal proportion of the participants would score in the expected direction for each of the conditions in the prediction using a One-Tailed Binomial Test (see Appendix A for further explanation of how the Binomial Test was used). For example, with the prediction of FR > BR and SR, p (the probability that the FR condition will be the slowest) will equal $^1/_3$, while q (the probability that one or both of the BR or SR conditions will be the slowest) is $^1/_3 + ^1/_3 = ^2/_3$. The results of the Binomial-Test are presented in Table 5.

The expected effect for the distractor being on the movement path of the hand is shown in an elevation of the Movement and Total Times for the FR condition in relation to the BR and SR conditions.

Table 4: Table showing the results obtained from One-Way Repeated Measures ANOVAs performed on the Reaction, Movement and Total Time measures.

° significant at the 0.10 level.
* significant at the 0.05 level

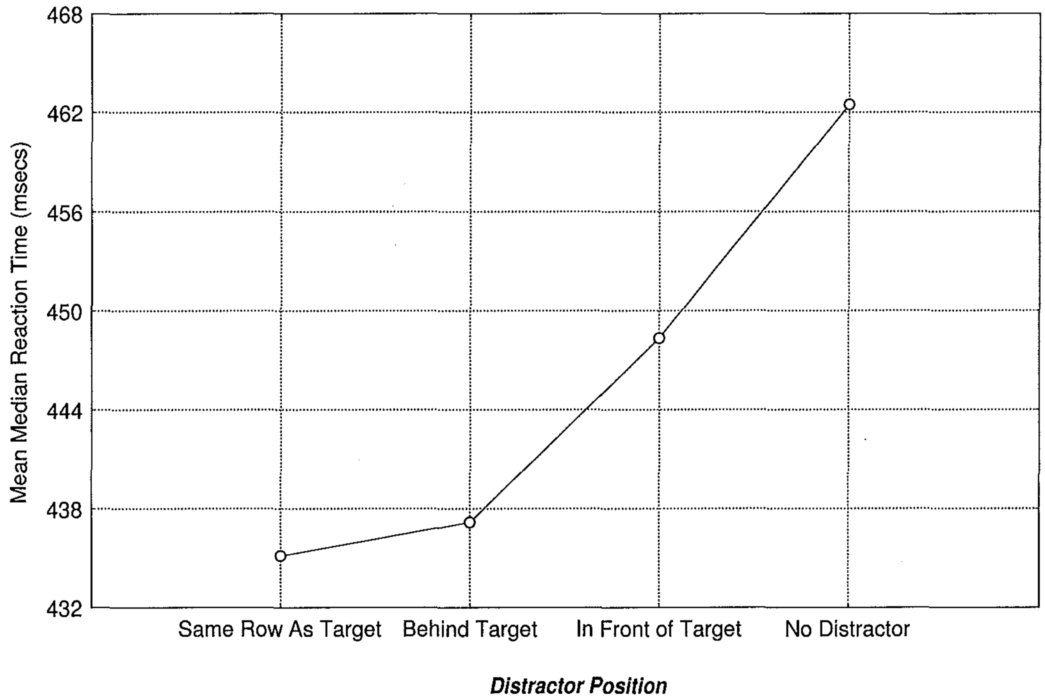
Measure	df	F =	*p* <
RT	3, 39	2.72	0.0576°
MT	3, 39	2.24	0.0986°
Total Time	3, 39	2.25	0.0693°

The prediction that a distractor on the same row as the target would produce greater interference than a distractor on a row behind the target is not supported by these results, with none of the time measures showing a significant number of participants displaying this effect.

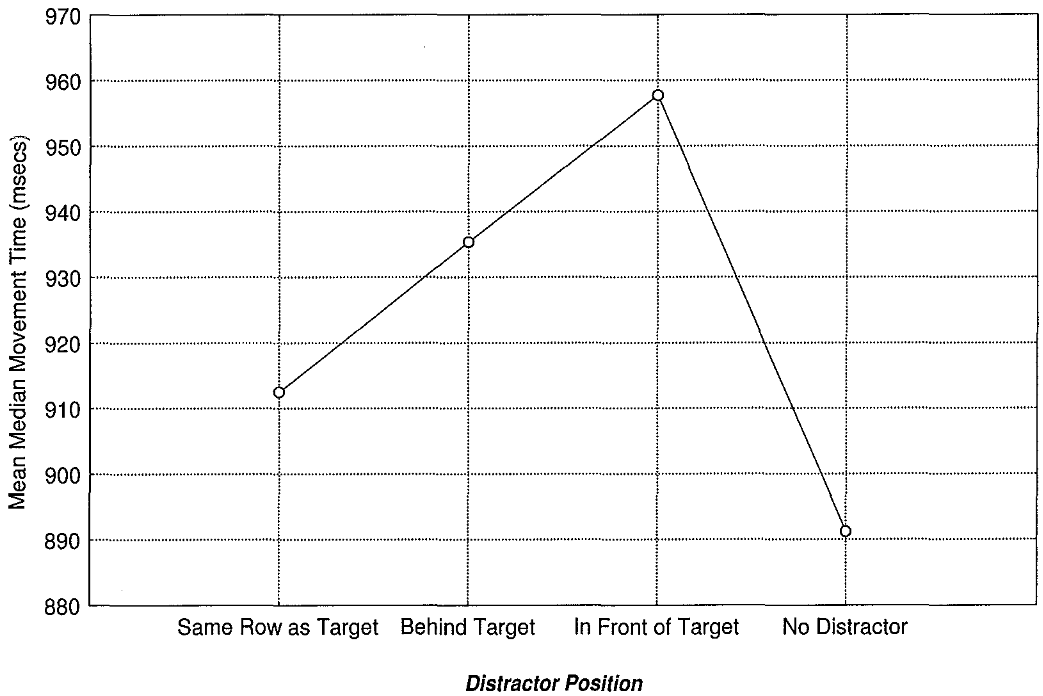
The most surprising result from the Binomial Tests is the strong trend in the opposite direction of the prediction for the ND condition. It was predicted that when the target was presented alone, total time would be faster than in all of the other conditions. This turned out not to be the case for both the Total and Movement Time measures. For both these measures, only one participant had a median time which was faster for the ND condition than for all the other conditions. Clearly, this represents a major departure from the results found in Tipper et al. (1992).

Table 5: Results for One-Tail Binomial Tests performed on the experimental predictions for each of the time measurements. The first number represents the number of participants conforming to the prediction (n=14).
° significant at the 0.10 level.
* significant at the 0.05 level

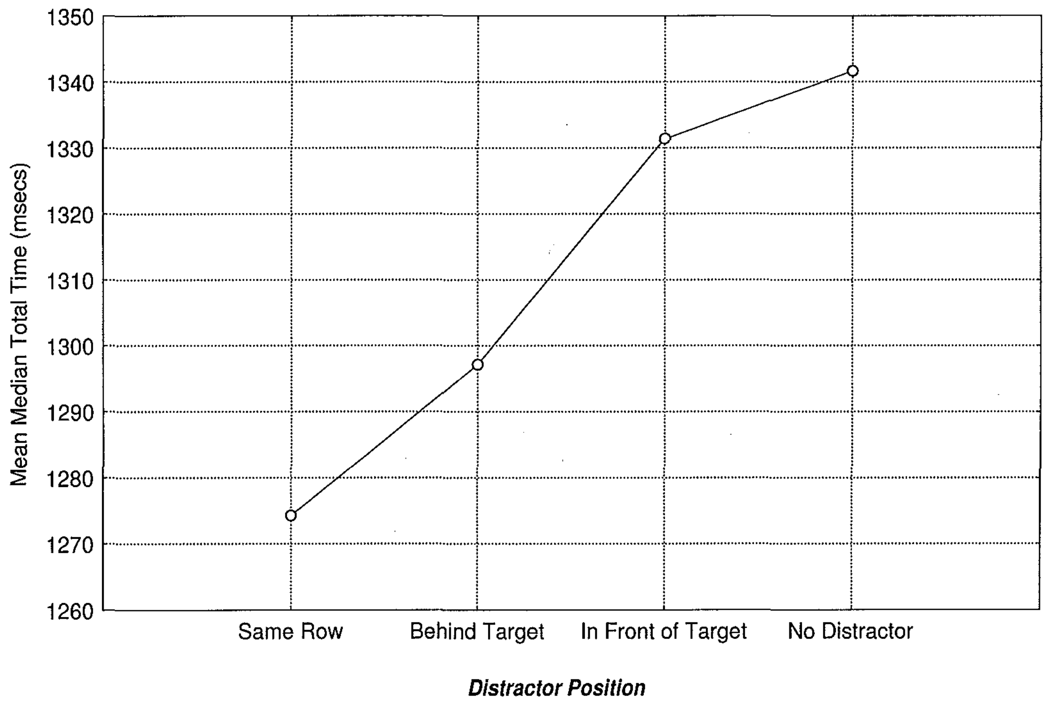
Prediction:	Total Time	RT	MT
FR > BR	10, $p < 0.090$°	10, $p < 0.090$°	13, $p < 0.001$*
SR > BR	5, *n.s.*	8, *n.s.*	5, *n.s.*
FR > BR and SR	8, $p < 0.058$°	7, *n.s.*	11, $p < 0.001$*
ND < all other conditions	1, $p < 0.008$°	5 , *n.s.*	0, $p < 0.001$*


468
462
456
450
444
438
432
Mean Median Reaction Time (msecs)
Same Row As Target
Behind Target
In Front of Target
No Distractor
Distractor Position

(a)


970
960
950
940
930
920
910
900
890
880
Mean Median Movement Time (msecs)
Same Row as Target
Behind Target
In Front of Target
No Distractor
Distractor Position

(b)



(c)

Figure 2: Mean median (a) Reaction, (b) Movement and (c) Total Time for each distractor position in Experiment 1.

Discussion

The results obtained in this study are consistent with the main prediction that participants would prove slower when the distractor was positioned on the reach path than when it was not. This effect manifested itself in the Movement and Total Time measures, with a significant proportion of participants performing more slowly in the FR condition than the SR and BR conditions. Such a finding is consistent with Tipper et al.'s (1992) results.

The other two predictions made were not supported. A distractor positioned in the Same Row as the target did not seem to lead to slower performance than a distractor located in the row behind the target. Tipper et al. reasoned that a distractor in the same row as the target will, on balance, be located on or near the reach path more often than a distractor situated behind the target. If the hypothesis concerning the effects of a distractor on the reach path is correct, then the same-row condition should prove slower than the back-row condition. However, informal observation of the participants' hand movements suggest that in the present study, the participants rarely, if ever, moved their hands over the distractor when it was positioned on the same row as the target. It is not surprising therefore that the same-row and back-row conditions resulted in similar response times.

The most surprising result was that the vast majority of participants did not respond fastest when the distractor was absent. Both Tipper et al. (1992) and Tipper et al. (in press) found that when the target was presented by itself, participants were much quicker to respond. Because of the robustness of this finding in other research, an account for why the present study fails to replicate it must be given.

The most fundamental difference between Tipper et al.'s (1992) study and the present experiment is the information needed to specify the grasping action. Because the distractor object used in Tipper et al. could not be displaced by the participant's limb during the reaching movement, very little information concerning properties of that object needed to be incorporated into an action plan for pushing the target button. In contrast, the distractor object in the present study could easily be dislodged. In order to grasp the target cube accurately, the participant needed to incorporate certain details about the distractor cube into their action plan, if only to avoid it. So by making it possible to dislodge the distractor cube, and emphasising that it was an error to do so, the salience to the successful completion of the grasping action of distractor attributes such as location and size was far higher in the present study than in that of Tipper et al. (1992).

The need for information regarding certain attributes of the distractor in the present study may account for the difference in results for the no distractor condition. To successfully complete the task without dislodging the distractor cube, the participant *must* know the location of the distractor in relation to the target and, to a lesser extent, its size[4]. Given the emphasis placed on avoiding the distractor, it may be that the participants use a reasonably conservative response strategy when the distractor was absent. In other words, participants may have taken a little more time in the no distractor condition to confirm that the distractor was indeed not present. This is in fact what several participants reported doing. In these cases, the participants said that they begin each trial by actively searching for the distractor. If their informal comments are correct, then it is not surprising that the no distractor condition exhibited an elevated level in comparison to the trials where the distractor was present. Any search when the distractor is present will terminate sooner than one when the distractor is absent. This of course assumes a serial rather than parallel search, but since the visual array was larger than could be fixated with a single saccade, this may be a reasonable assumption to make.

Further support for this explanation can be derived from the Reaction and Movement Times shown by the participants when the distractor was absent. Graph A in Figure 2 shows that Reaction Times were slower on average when the distractor was absent than when it was present at any location. This is consistent with an exhaustive search of the stimulus board for the distractor. Movement Times, on the other hand, were quicker on average when the distractor was absent (see Graph B in Figure 2). This suggests that once the participants had failed to find the distractor in their search, they no longer were constrained by the need to avoid the distractor, and so could perform the reach movement quickly. This interpretation of the Reaction and Movement Times has to be qualified by the comments made earlier on the difficulties of determining how much of Reaction Time is actually incorporated into the Movement Time measure. Nonetheless, the results here are suggestive, and deserve further investigation. They also point to the potential usefulness of dividing response time into its reaction and movement components.

Such an explanation of the difference between the no distractor condition in this and Tipper et al.'s

[4] Unless the participant adopts an extreme response strategy, such as not discerning the distractor's location before reaching or always reaching in an upward arc motion so as to avoid any object between the start button and the target. These two strategies are unlikely to have been adopted by any of the participants in this study, however. None of the participants displayed the elevated error rates that would accompany the first strategy, while the construction of the board enclosure limited the participants' ability to move

(1992) study is of course tenuous. The current study simply does not provide enough information for a detailed model or theory to be outlined. For example, it may be that rather than the no distractor trial times being elevated, the distractor present conditions' times may have been reduced in comparison. The contention here is although the results do not provide unambiguous support for any particular theory, they do suggest that making it possible to dislodge the distractor materially changes the nature of the task. If this is the case, it is an important finding for two reasons. Firstly, it adds to the other evidence supporting an action-oriented approach to the study of visual attention by demonstrating how facets of a phenomenon which would have remained undetectable using traditional methods emerge when the perception-action cycle is studied intact. And secondly, it highlights the context-dependent nature of attention, and the dangers of assuming that results obtained in one experimental setting will generalise to others.

The elevation of response times when the distractor was absent also leads to further problems of interpretation. As was done in this and Tipper et al.'s (1992) experiments, the No Distractor condition was used as an index of interference. This practice is predicated on the assumption that, on the basis of results from previous research, response times will be slower when a distractor is present, and that this represents the cost of the extra processing required to deal with distractor's presence. Clearly, this situation no longer holds with the results obtained in this experiment, and highlights still further the concerns mentioned about the generalisability of findings from earlier studies.

In summary, the most interesting finding of this study was that the no distractor condition was not the fastest of all the conditions. This is quite at odds with findings by other researchers using similar methodologies. This incongruity with other research may be due to the use of distractor objects which could be dislodged during the reach movement, and the negative consequences of doing this. The next study examines this tentative explanation further and attempts to test its plausibility.

their arm much beyond the height required to successfully complete the task.

Experiment Two

The purpose of this study was to test the hypothesis that having a distractor which can be dislodged was the primary cause of the discrepant time measures in the distractor absent condition. In order to do this, a thin copper plate painted the same colour as the distractor cube in the previous experiment was employed. Because of its thinness, the plate provided little obstruction to movement and was virtually impossible to dislodge.

Determining whether the 'dislodgability' of the distractor object causes a marked change in performance is, as discussed in the previous experiment, of keen interest to researchers in visual attention. If it can be shown that the postulated effect is a reality, it highlights the importance of striving towards more and more realistic analogues of the real-world tasks confronting attention. Without adopting an action-oriented approach, this effect would simply not be discovered.

The design of this experiment is based on that employed in Tipper et al. (in press). In this series of experiments, Tipper et al. substituted the buttons and lights used in their 1992 experiments with coloured cubes. As in Experiment 1 of this series, Tipper et al. used two differently coloured cubes of equal size. At the beginning of each trial the participant was given a visual cue informing them which of the cubes was the target for that trial. In this way the two cubes had no fixed identity. Depending on the trial, either could be a target or distractor. Tipper et al. (in press) also decreased the number of locations used from a 3 x 3 grid to a 2 x 2, or from a total of 9 to 4 locations.

The results they obtained using this altered design essentially replicated their previous findings. Response times were slower when the distractor was on the movement path[1], as in their earlier experiments. However, since the cubes were reasonably large, this could plausibly have been explained by the participant having to avoid the object. Tipper et al. (in press) also replicated their original finding that response times were significantly faster when the distractor was absent. This presents something of a problem for the hypothesis that the lack of this effect in Experiment 1 was due to the dislodgability of the distractor, since Tipper et al.'s (in press) studies used distractor cubes which were bigger than those in Experiment 1, and so probably easier to dislodge. This issue will be revisited when the results of this experiment are discussed.

The use of a design similar to that employed by Tipper et al. (in press) conveys a number of advantages. The most attractive of these is that because of the smaller number of locations, an analysis of each location separately becomes feasible. This makes it easier to see whether any interesting effects, such as left-right asymmetries, occur at some locations but not others. It also simplifies the experimental procedure and eliminates the need for filler trials. It is important to note, however, that this experiment does differ from Tipper et al.'s (in press) in significant ways. As mentioned earlier, the distractor object was actually a flat copper plate painted green, not a cube. As such, the distractor and target object identities remained fixed. The red cube was always the target, and the green plate was always the distractor. Also, the

[1] By employing a hand tracker, Tipper et al. (in press) determined that the distractor was on the reach path when it was positioned in

salience of the distractor to the task was manipulated in this experiment by varying whether or not the participants were told if it was an error to dislodge the distractor, and to thus avoid, or simply told just to ignore it altogether.

A new time measure is also introduced in this experiment. The is the Inter-Trial Interval, or the time that elapses from the conclusion of one trial and the commencement of the next. These are intended to provide some form of consistency check. A finding of significant differences in the Inter-Trial Times across conditions intimates that the other time measures may have been contaminated by the variation in these times.

If the dislodgability of the distractor object was the critical determinant of the results in Experiment 1, it is to be expected that response times will be quickest when the distractor is absent, as is normally found. It is also predicted that instructing the participants to avoid the distractor will lead to results similar to those found in Experiment 1.

Method

Participants

Sixteen student volunteers (11 females) took part in this study. Ages ranged from 17 to 31 years (mean: 24 years). All participants were right-handed and had normal or corrected-to-normal vision according to self report. No participants reported any deficiencies in colour perception. All of the participants were required to be at least 165 cm tall due to the arm length necessary to comfortably reach the fourth row on the stimulus board. A small cash payment of $8.00 was made for their participation. Participants were randomly assigned to one of the two instruction groups. Each group ultimately contained 7 participants.

Apparatus

The equipment used was the same as for Experiment 1, with the following modifications. A sheet of black cloth was laid over the stimulus board, which prevented the participants from viewing the object locations that were not used in this study. Square holes were cut into the cloth at the four positions where the cubes could be placed. The holes were positioned over the four corner locations on the stimulus board, as shown in Figure 3 (overleaf). The centre of each location was 30 cm from the other location in the same row or column. These distances are close to those used in Tipper et al. (in press). A faint white chalk mark was made at that centre of the locations to provide a visual fixation point for the participants. The chalk mark was equidistant from all of the locations. The centre-most start button was used. A flat plate of copper measuring 4 cm x 4 cm square was substituted for the green distractor cube. The copper plate was painted

the location directly in front of the target.

the same green colour as the distractor cube in Experiment 1. Because of the thinness of the plate, it was extremely difficult to displace it accidentally.

Design

A mixed one-factor within one factor between subjects design was used. The relative location of the distractor cube to the target was the repeated measures variable, while the instructions given to participants formed the between groups variable. Participants' Reaction, Movement, and Total Times, along with error rates and Inter-Trial Time, were the dependent measures.

Since there were markedly fewer locations in which the objects could appear in this experiment than in the first, every location was used for later analysis. In other words, there were no filler trials. The trial sequences consisted of four blocks. Each block in turn was made up of 32 trials. Every combination of target and distractor location pairings was used twice in each block. When the target was located in position 1, there were two trials each for the distractor in position 2, 3, 4 and two with the distractor not present. This makes a total of 8 trials for each block in which the target appeared at a particular position. Since

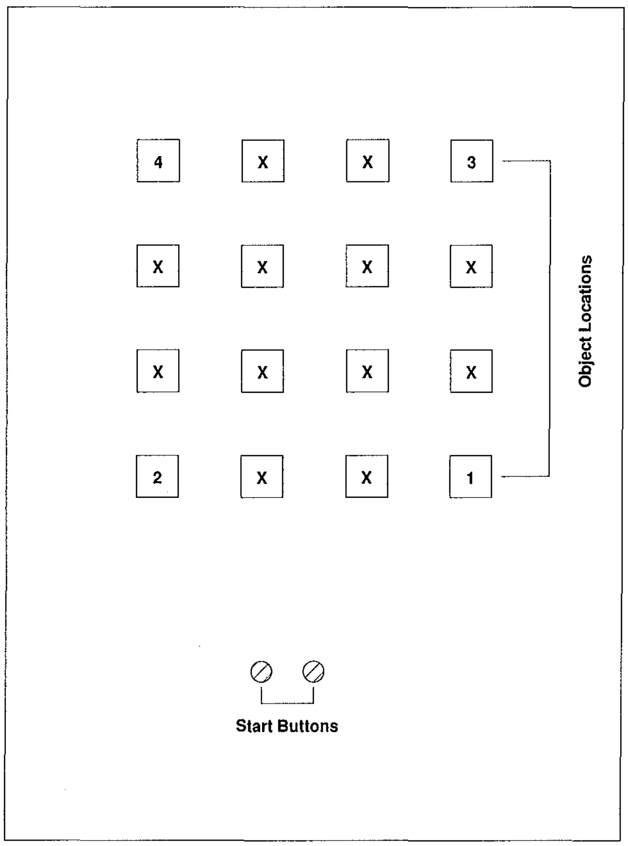


Figure 3: The logical layout of the stimulus board in this experiment. 'X' denotes that that location was not used. Drawing is only approximately to scale.

there were 4 positions, this gives the 32 trials per block. The trials were randomised within each block by computer and then joined together to give 4 blocks, or a total of 128 trials. This was identical to the trial generation method used by Tipper et al. (in press), with the exception that they used 5 blocks instead of 4.

Procedure

The procedure used in this experiment was exactly the same as that used in Experiment 1 with two exceptions. During the practice trials the participants were required to memorise the location of the chalk mark in the centre of the board and to fixate their gaze at that point prior to the commencement of each trial. This was done to ensure that the participants had focussed their gaze at approximately the same place on the board at the start of each trial.

Also, the effect of the type of instructions given concerning the nature of the distractor were varied across participants. One group of participants were given the standard instructions that they were to avoid the distractor, and that dislodging it was counted as an error. The other group were simply instructed to ignore it. They were not informed whether or not dislodging the distractor would count as an error. Both groups were still told that releasing the start button before the light inside the enclosure had come on would constitute an error.

Results

Of the 16 participants who took part in the experiment, 2 were excluded from analysis (1 female). This was due to an extreme level of errors produced by one participant, and the other presenting symptoms consistent with recent alcoholic intoxication. This left 14 valid data sets. Trials on which an error occurred were omitted from further analysis. Otherwise, all valid trials were analysed. The results for the Time Measures are shown in Figure 4 (page 39).

The probability of a participant making an error on any particular trial is shown in Table 6 (overleaf). These probabilities are broken down according to position, condition and the instruction set given. The pattern of these probabilities, when combined with the time measures shown in Table 6, do not conform to the pattern predicted by the Speed-Accuracy Tradeoff, and so tend to rule that out as a possible explanation of the results. It should be noted that, as more fully considered later, there are less ways to make an error when the distractor was absent, since it obviously could no longer be dislodged.

ANOVAs

Mixed design ANOVAs were performed for each position on the median time measures obtained for each participant. The Instruction Condition (Avoid vs Ignore) was used as a between groups factor, while Distractor Position constituted the repeated measures factor. The results of these ANOVAs are summarised in

Table 6: The probability of making an error on a trial broken down by position, condition and instruction set.

Position	Condition	P(Error) Avoid	P(Error) Ignore
Front Right	No Distractor	0.000	0.000
	Same Row	0.000	0.002
	Back Row	0.003	0.006
Front Left	No Distractor	0.000	0.000
	Same Row	0.001	0.003
	Back Row	0.001	0.002
Back Right	No Distractor	0.000	0.001
	Same Row	0.001	0.002
	Directly In Front	0.000	0.000
	Diagonally In Front	0.000	0.000
Back Left	No Distractor	0.001	0.001
	Same Row	0.001	0.006
	Directly In Front	0.001	0.002
	Diagonally In Front	0.001	0.001

Appendix A. Because of low power and participant numbers used in this study, a more liberal alpha level of 0.10 was used to assess significance. All post hoc comparisons were performed using Tukey's HSD.

Front Right

The only significant effect reported by the ANOVAs for this location was a main effect for Distractor Position on Inter-Trial Interval, $F(2, 24) = 26.37$, $p < 0.000$. For reasons which will discussed later, finding significance for the Inter-Trial Interval measure is difficult to interpret, and unlikely to be particularly meaningful. Therefore findings of significance for the Inter-Trial Interval will be reported, but not subjected to post hoc analyses.

Front Left

The only significant results revealed by the ANOVAs were obtained in Inter-Trial Interval, with a main effect for Distractor Position, $F(2, 24) = 12.46$, $p < 0.000$, and an interaction effect, $F(2, 24) = 3.73$, $p < 0.039$.

Back Right

Only one statistically significant result was found at this location. This was a main effect for Distractor Position on Inter-Trial Interval, $F(3, 36) = 2.78$, $p < 0.055$.

Back Left

As with the Back Right location, the only statistically significant result found was a main effect for Distractor Position on Inter-Trial Interval, $F(3, 36) = 7.84, p < 0.000$.

Trends

The graphs for Movement and Total Time in Figure 4 (overleaf) display an interesting asymmetry between the participants' responses to targets on the left side of the stimulus board and their responses to targets on the right. It appears that participants responded quicker when targets were presented on the right side of the board.

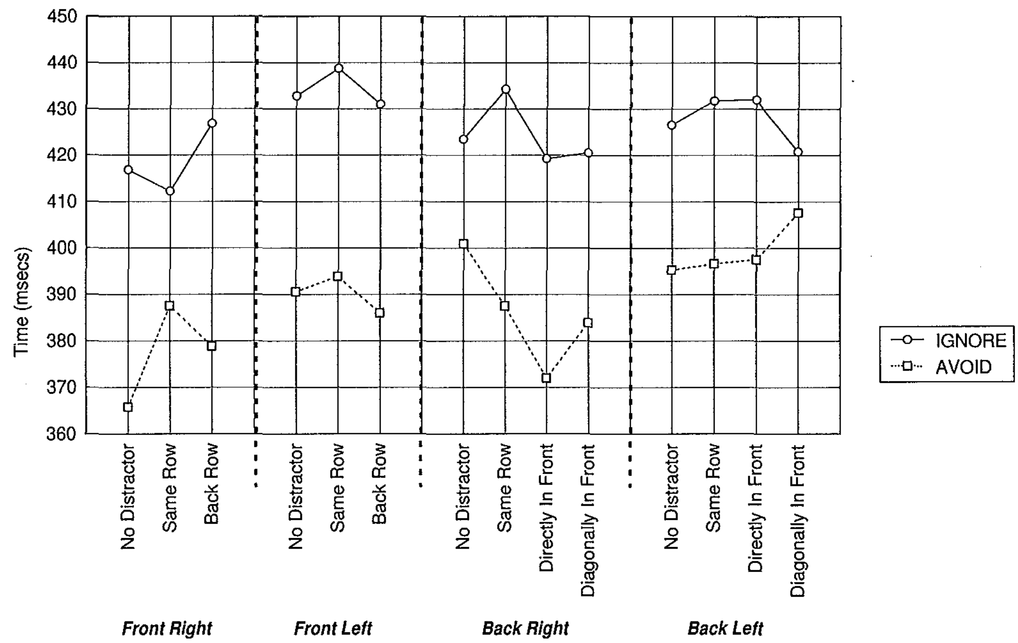
Correlations

Pearson's r correlations were calculated between Inter-Trial Interval and the other time measures in order to further assess any impact the interval between trials may have had on performance. These correlations are presented in Table 7 below.

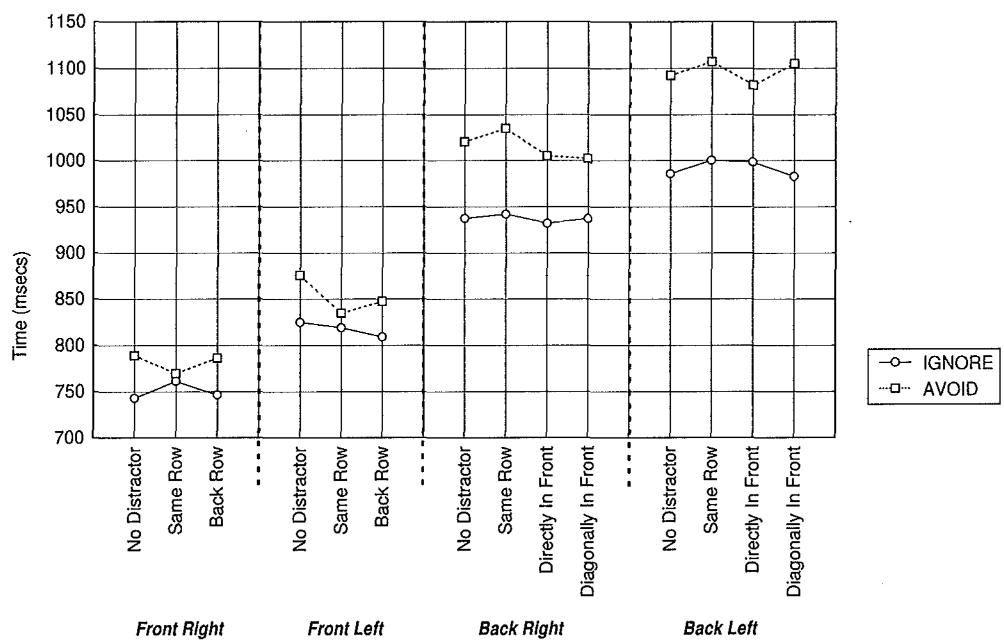
Significant positive correlations were found between Inter-Trial Interval and all of the other time measures for the Back Right position when the distractor was absent. Similarly, significant positive correlations were also obtained between Inter-Trial Interval and both Movement and Total Time at the same position when the distractor was in the same row. As with the significant findings in the ANOVAs for Inter-Trial Interval, these correlations are difficult to interpret, although the relative scarcity of them suggests that the time interval between trials was not a major influence on participant performance. This issue will be discussed in greater detail in the discussion for this experiment.

Table 7: Pearson's r correlations between Inter-Trial Interval and the other time measures. Correlations significant at the 0.05 alpha level are marked with an *.

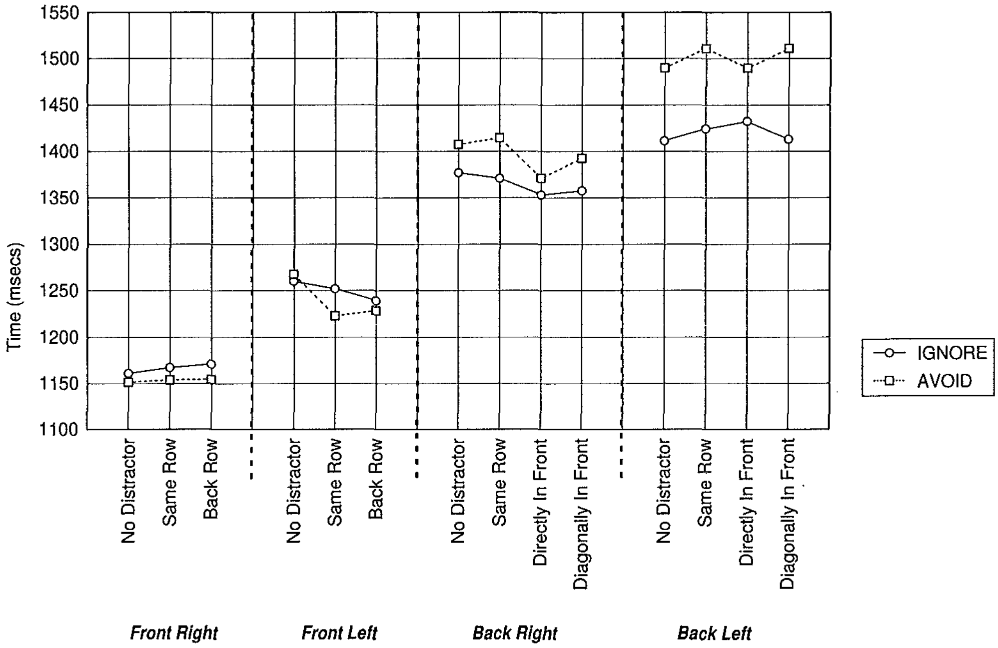
Location	Condition	ITI and RT	ITI and MT	ITI and Total Time
Front Right	No Distractor	0.18	0.20	0.22
	Same Row	-0.08	0.26	0.17
	Back Row	0.07	0.34	0.24
Front Left	No Distractor	0.39	0.44	0.43
	Same Row	0.26	0.27	0.27
	Back Row	0.10	0.29	0.25
Back Right	No Distractor	*0.66**	*0.64**	*0.68**
	Same Row	0.44	*0.66**	*0.66**
	Directly In Front	-0.04	0.06	0.05
	Diagonally In Front	0.02	0.33	0.28
Back Left	No Distractor	0.08	0.51	0.43
	Same Row	0.19	0.30	0.29
	Directly In Front	0.13	0.47	0.39
	Diagonally In Front	0.25	0.25	0.27


Time (msecs)
450
440
430
420
410
400
390
380
370
360
No Distractor
Same Row
Back Row
Directly In Front
Diagonally In Front
Front Right
Front Left
Back Right
Back Left
IGNORE
AVOID

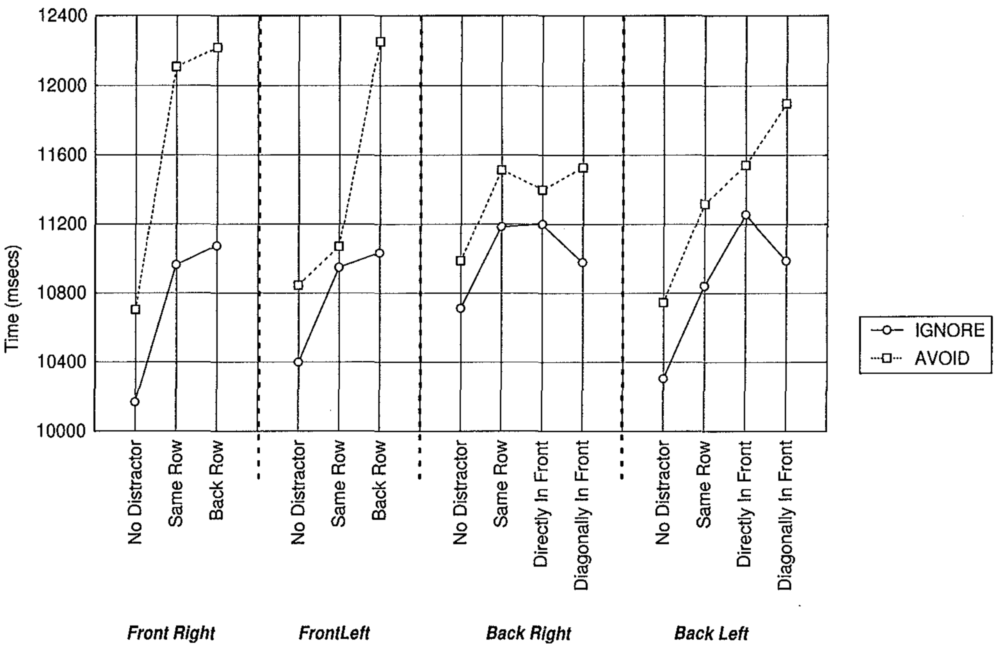
(a)


Time (msecs)
1150
1100
1050
1000
950
900
850
800
750
700
No Distractor
Same Row
Back Row
Directly In Front
Diagonally In Front
Front Right
Front Left
Back Right
Back Left
IGNORE
AVOID

(b)



(c)



(d)

Figure 4: Graphs depicting the mean median (a) Reaction, (b) Movement, (c) Total and (d) Inter-Trial Time for each distractor position in both of the instruction manipulations in Experiment 2.

The Binomial Tests

Overall, the mixed design ANOVAs failed to show any significant effects except in the Inter-Trial Interval time measure. The lack of significant results is not surprising, however, when the results of a post hoc Power Analysis are considered (see Appendix A for these analyses). Given that the highest level of power achieved in this study was 0.2535, it is quite astonishing that any significant effects were found at all.

In order to get a better insight into the individual patterns of results, Binomial Tests were performed as in Experiment 1. The two main predictions from Experiment 1 were tested for each location. These were that the time measures would be quickest when no distractor was present, and that when a distractor was present, the time measures would be slowest when the distractor was on the movement path (in this study, directly in front of the target).

Table 8 shows the results of these Binomial Tests in the two instruction manipulation conditions for each time measure, except Inter-Trial Interval. One of the more striking features of these results was the low number of participants responding slower when the distractor was situated on the movement path (FRF > SR and FRD). Whereas in Experiment 1 there was a definite trend towards slower response times in this situation, this trend was conspicuously absent in the results for this study. This suggests that the use of an object that was difficult to dislodge in this study led to the participants essentially ignoring the distractor, regardless of the instructions they were given.

The results of the Binomial Tests examining the prediction of quicker responses when the distractor was absent were more ambiguous. While the number of participants conforming to this prediction was low, they were not as dramatically so as found in Experiment 1. This may in part be due to the lack of participants in this study, but may also reflect some rebounding in the No Distractor condition compared to the results of Experiment 1.

Table 8: Results on Binomial Tests for each Location. The first digit represents the number of participants conforming to the specified prediction (n = 7), with the associated *p* value following.

ND = No Distractor condition.
FRF = Distractor directly in front of target.
FRD = Distractor diagonally in front of target.
SR = Distractor on same row as target.

° significant at the 0.10 level.
* significant at the 0.05 level

		Front Right	*Front Left*	*Back Right*		*Back Left*	
	Instruction	**ND < ALL**	**ND < ALL**	**ND < ALL**	**FRF > SR and FRD**	**ND < ALL**	**FRF > SR and FRD**
RT	**Avoid**	2, *n.s.*	2, *n.s.*	1, *n.s.*	2, *n.s.*	1, *n.s.*	2, *n.s.*
	Ignore	1, *n.s.*	4, *n.s.*	2, *n.s.*	0, $p < 0.046$*	3, *n.s.*	1, *n.s.*
MT	**Avoid**	2, *n.s.*	1, *n.s.*	1, *n.s.*	1, *n.s.*	0, $p < 0.001$*	1, *n.s.*
	Ignore	4, *n.s.*	2, *n.s.*	2, *n.s.*	3, *n.s.*	1, *n.s.*	3, *n.s.*
Total	**Avoid**	3, *n.s.*	2, *n.s.*	2, *n.s.*	2, *n.s.*	2, *n.s.*	1, *n.s.*
	Ignore	3, *n.s.*	1, *n.s.*	1, *n.s.*	1, *n.s.*	3, *n.s.*	4, *n.s.*

Discussion

Because of the low number of participants, and consequently the modest power of this study, any conclusions drawn from the results are tentative. Nonetheless, there are a few trends within the data that suggest interesting avenues for further investigation.

As was alluded to earlier in the results section, very little can be read into the findings of significant differences between the different distractor positions in the Inter-Trial Intervals. Differences in Inter-Trial Intervals may, for instance, provide the participant with information on the presence of a distractor on an impending trial. While initially plausible, it is unlikely that Inter-Trial Intervals themselves played a major role in determining participant performance. Inter-Trial Intervals were themselves dependent on errors, since making an error led to a longer than usual delay between trials. Errors were themselves unevenly distributed across distractor positions. Indeed, most errors were caused by the experimental apparatus falsely registering that the distractor plate had been dislodged by the participant, due to the force of the participant's movement causing the plate to wobble. While this was a relatively rare occurrence, and trials on which this did occur were removed from further analysis, this type of error did account for the vast majority of errors recorded. Note that these errors could not occur when the distractor was absent, which explains the uneven distribution of errors across distractor positions. This uneven distribution of errors was most probably the cause of the differences between distractor position Inter-Trial Intervals. The low number of significant correlations obtained between Inter-Trial Interval and other time measures also supports this conclusion.

The most interesting aspect of the results obtained in this study is the low number of participants who displayed the expected slowing of response measures when the distractor object was situated on the reach path. Experiment 1 showed a significant effect for this, but the effect appears absent in the results from this study. Given that the distractor no longer presented a physical obstruction to reaching the target cube, this seems readily explicable; the participants were no longer required to move around the distractor object, and this was therefore reflected in the response times. This does not sit well with Tipper et al.'s (1992) findings however. Recall that they used buttons as the objects to be responded to and red and yellow lights to signal which buttons were target and distractor objects, respectively. The distractor light and button did not present a physical obstruction to reaching the target object in their study, yet they found a marked impairment of response time when the distractor light and button were positioned on the reach path. The reason for this inconsistency between the two studies may lie in the different potentials for action that the distractor objects had. In Experiment 2, the distractor plate was never acted towards, whereas in Tipper et al.'s experiment, a button which was a distractor on one trial became a target on another. Thus all of the objects in Tipper et al.'s experiment were potential target objects, and hence potential competitors for action. This situation did not occur in Experiment 2; the distractor object was never a target. In other words, the objects in Tipper et al.'s experiment had dual identities which altered from trial to trial according to the colour of the light in front of it, while the objects in the present study did not.

More difficult to interpret are the results of the Binomial Tests for the prediction that the fastest response times would be obtained when the distractor was not present. One the one hand, the number of participants showing results in accordance with this prediction are low, which again goes against previous research findings. But on the other hand, the number of participants is not significantly low, as in Experiment 1, and are around the level that would be expected by chance. It is here that the small participant numbers make explication of the results particularly difficult. Since the number of participants is small, the difference between a significant result and one at chance level is only one or two participants. This leaves a great deal of room for random variation to mask any interesting effects which may be present.

The phrasing of the instructions given to the participants appeared to have little impact on their performance. Regardless of whether they were told to avoid the distractor object, and that dislodging it was an error, or to simply ignore it, they seemed to treat the task in much the same way. Informal verbal reports by participants suggested that most of them simply ignored the distractor plate. Many reported that they did not even notice if it was present or not. The results do provide some measure of support for the participants' self-observation. The reduction in the proportion of participants conforming to the predicted slowing of response times when the distractor was on the reach path from Experiment 1 suggests that the distractor did indeed lack the salience it had in the previous study. The near-chance results of the Binomial Tests on the prediction that the fastest response times would be obtained when no distractor was present also tend to support the hypothesis that the participants simply ignored the presence (or absence) of the distractor.

The graphs in Figure 4 also provide further support for the idea that the participants essentially ignored the distractor. With few exceptions, performance across distractor positions was very similar. Likewise, the difference in time measures between participants in the Avoid and Ignore conditions is not large. The lack of any marked impact by the position of the distractor, or its absence, and the instructions given to the participants points quite strongly to the distractor's lack of salience in the performance of the reaching and grasping task.

Further analysis of the graphs in Figure 4 reveals an interesting asymmetry in the participants' Movement and Total Times for which side of the board that the target was presented on. Targets which appeared on the right hand side were responded to faster than those presented on the left side. This is in accordance with results obtained by Tipper et al. (1992). The difference in response times can readily be accounted for by the changes in posture required to perform the reaching movement. Since the participants were right-handed, reaching towards targets on the left side of the board required that they twist their torso in order to extend their arm. This twisting motion was not required when the target appeared on the right side of the board. Consequently, reaching towards a target on the left side of the board took slightly longer than reaching for a target on the same row but on the right hand side.

Taken together, the results of this study seem to suggest that the participants effectively ignored the presence of the distractor object. If this is the case, it reiterates the point made in Study 1 concerning the differences between using real world versus artificial tasks to study human performance. Here, using a type of task which is common in everyday experience and our evolutionary history, a different set of results has been obtained to those which have been found using the traditional techniques of Cognitive Psychology. In this instance, the dislodgability of the distractor object appears to have had a major effect on the interference it produces. Clearly, such an effect could not easily be discovered using the conventional two-dimensional displays with alphanumeric symbols in place of objects. It requires an action-oriented approach which keeps the perception-action cycle intact.

There is another possible explanation of the results obtained which is closely related to the one given. If one looks at the task in terms of the affordances for action present (Gibson 1979), it becomes immediately apparent that the distractor and target objects differed not only in terms of their dislodgability, but in the actions they afforded. The target object, a cube, afforded grasping and picking up. The distractor, on the other hand, was simply a thin copper plate which did not afford picking up. This is in contrast to Study One, where both objects were easy to uplift. It may have been the difference in the affordances of the objects, and not just the dislodgability of the distractor, which led to the participants ignoring the distractor. Even if this were the case, however, it still supports the main point made in this study, which is the usefulness of an action-oriented approach.

In summary, the results of this study suggest that in a reaching and grasping task, when the distractor is either not easily dislodged or affords markedly different actions to the target object, the distractor is effectively ignored. Neither it's presence or absence, nor its positioning on the movement path, seem to have any marked impact on temporal performance. Further research is needed to explore these issues, using a more powerful experimental design with greater participant numbers.

General Discussion & Conclusions

The goal behind the two experiments in this study was simply to examine what happens when the task of selecting a target in the presence of distractors is performed using real objects and natural movements. This task has been used quite extensively in Cognitive Psychology to study the mechanisms underlying attentional selection, but typically using very constrained settings. These usually consist of alphanumeric stimuli presented via a computer screen for extremely short durations, which the participant is required to respond to by pressing a button to indicate the presence or location of the target stimulus. This sort of situation, while quite common, is not particularly representative of the kinds of tasks people perform in everyday life, nor of the ecology in which attentional selection evolved. By using a task which is realistic and natural, it was hoped to see whether the results of previous research which used highly constrained and artificial experimental designs would generalise to more ecologically valid situations, as is generally assumed.

Experiment 1 extended on the basic design employed by Tipper et al. (1992). Tipper et al. in turn attempted to extend the more traditional techniques in Cognitive Psychology by using coloured lights as stimuli, and having participants reach out and press the button located behind the target light. While Tipper et al.'s study replicated the usual finding that response is quicker when the target is presented by itself, they also found that placing a distractor between the participant and the target led to greater interference in response time than a distractor was presented at any other position. This was a novel finding which could not be accounted for by such factors of physical obstruction or visual occlusion by the distractor. Clearly, there are important implications from these results for how the reach to grasp movement, and action in general, is represented in the cognitive system. These results could not have been obtained without using a natural action task which preserved the perception-action cycle.

Experiment 1 went beyond this by having the participants reach and grasp cubes, which were themselves the stimuli. Although the results of this study did not reach statistical significance, the trends evident in the data were consistent with Tipper et al.'s (1992) findings of an enhanced interference effect when the distractor was situated on the reach path. Unlike Tipper et al.'s study, however, the distractor cube did present a physical obstruction to the target, and so the possibility that the exaggerated response times when the distractor was in front of the target were simply due to the time required to avoid the distractor could not be excluded.

The most intriguing result from Experiment 1 was that response times were not quickest overall when the target was presented by itself for the vast majority of participants. Such a finding is at odds with both Tipper et al. (1992) and the literature in general. A possible explanation for this is that because the distractor could now be dislodged, the participants used a more conservative response strategy. In order to complete the movement successfully, the participants needed to know where the distractor was positioned, so that certain of its properties could be incorporated into the action plan and the distractor avoided. A self-terminating search for the distractor could be one possible explanation for the increased response times when the distractor was absent. In other words, because the participant needs to know where the distractor is to complete the task, they scan the visual field for it. Scanning would obviously take less time when the distractor is present than when it is absent, since the search would terminate sooner. This hypothesis gains further support from the elevation of Reaction Times coupled with the relatively quick Movement Times shown in the No Distractor condition, which suggest that participants may have employed some form of search and, having discovered that the distractor was absent, then been able to reach for the target more quickly than if a distractor was present.

To examine further the hypothesis that having a distractor which can be dislodged was a major determinant of the unusual finding in the No Distractor condition, Experiment 2 used a thin copper plate painted green as a distractor, and divided the participants into two groups according to the instructions they were given. By using a distractor which was virtually impossible to dislodge, any effect due to having a dislodgable distractor should have been removed. The salience of the distractor to the task was also intended to be controlled by manipulating whether the participants were told to actively avoid the distractor or simply ignore it. Furthermore, the number of possible target and distractor locations was reduced from 9 to 4, as used in a similar experiment by Tipper et al. (in press). This reduction in the number of locations allowed for more in-depth data analysis which was not feasible when using 9 locations.

The results from this experiment proved to be interesting, yet inconclusive. What was obtained was essentially flat response times across conditions. This suggests that the participants ignored the distractor, regardless of whether it was present or what they were instructed. While such a result is consistent with the hypothesis that the elevated response times in the No Distractor condition of Experiment 1 were due to the distractor being dislodgable, it does not rule out other explanations. For example, the results could simply be taken at face value. Showing that the participants ignored the distractor could just point to the flexibility of the attentional system in selecting an efficient response strategy. Yet, as will be discussed later, even such an apparently trivial finding has some interesting implications for the results of previous research.

There are a number of discrepancies between the two experiments performed in this study and the experiments by Tipper et al. (1992) and Tipper et al. (in press), on which they are based, which need to be addressed. Aside from the differences in findings for the response times in the No Distractor conditions, the question of why Tipper et al. (1992) found an interference effect for a distractor on the movement path, despite the distractor light presenting no visual of physical obstruction, while Experiment 2 in this study did not, must be examined. A possible explanation for this was given earlier in the discussion for Experiment 2. In Tipper et al. (1992), an object which was a target in one trial would be a distractor on another. Thus, distractor objects were still potential competitors for action, and the interference effects found for when they were positioned on the movement path may reflect this. This explanation obviously needs to be tested empirically. One possible method for doing this would be to use slightly thicker plates than were used in Experiment 2 as both the target and distractor objects, and vary which had to be picked up from trial to trial.

Another intriguing discrepancy between the experiments in this study and those of Tipper et al. (in press) is that Tipper et al. (in press), despite using dislodgable objects for the distractor, did not find elevated response times when the distractor was absent. Clearly, this is problematic for the explanation of elevated response times in the No Distractor condition given earlier. A possible account for this discrepancy may lie in the difference between the number of locations present in Experiment 1 of this study and those used in the experiments by Tipper et al. (in press). Whereas a 3 x 3 grid of locations was used in Experiment 1, Tipper et al. only used a 2 x 2 array. This means that there were only 3 potential distractor locations for Tipper et al.'s study, but 8 for Experiment 1. Although the physical dimensions of the studies' arrays were very similar, Experiment 1 contained nearly three times the number of possible distractor locations. If the participants undertook a search for the distractor by location to confirm that it was not present, then it is possible that there simply were not enough locations in Tipper et al. for this effect to become evident. However, because of the larger pool of potential distractor locations in Experiment 1, time measures may have been elevated to such a level that the time taken by the location search was equal to or greater than any interference caused by the presence of a distractor object. Again, this explanation needs to be tested by performing experiments in which the number of locations present is systematically varied.

In summary, the experiments presented in this study, while not providing enough information to build a detailed theory on, or definitively rule out alternative explanations of the results, suggest a number of possible lines of research. Clearly, further research is required to determine whether the reasons given above are adequate to account for the discrepancies between this study's results and those obtained by Tipper et al. (1992) and Tipper et al. (in press).

But the results of this study also suggest other interesting research questions, including the one which was originally intended to be the focus of this thesis. What happens when the distractor and the target afford different actions? For example, what if the distractor affords pushing while the target affords grasping? Would this lead to diminished interference by the distractor? The answers to these questions are important for our understanding of just how the actions are represented by the cognitive system. Are objects represented in terms of the actions they afford by perceptual systems involved in the on-line control of action, as researchers such as Jeannerod (1993; 1994) and Goodale & Milner (1992; Milner & Goodale 1993) argue? Does the brain maintain different representations for an object simultaneously according to the information requirements of the current behavioural goal, as suggested by Tipper, Weaver, & Houghton (1994)? And what sort of coordinate system is used to represent actions? Is it simply based on the retinal projection, three-dimensional space centred on the perceiver (Downing & Pinker 1985) or independent of them (Maylor 1985), or are actions represented in terms of the path of action itself (Tipper et al. 1992)? Or are all of these coordinate systems used by the brain, depending on the task requirements and, if so, what circumstances dictate which of these systems are used? These are questions which an action-oriented approach can help provide answers to.

The answers to these kinds of questions are interesting not just for what they reveal about how the cognitive system represents actions, but also for the potential usefulness this information may have for optimising the ergonomics of human-machine interfaces. The layout of such interfaces can be optimised to more appropriately fit the nuances of the perceptual-motor system, thereby enhancing efficiency and reducing errors made by users. In a society where interaction with technology is becoming an increasingly important part of everyday life, designing interfaces which incorporate basic findings on the representations utilised in transactions with the environment is growing more important.

Wider Theoretical Implications

This thesis has argued strongly for the necessity of an action-oriented approach to the study of visual attention, not as a replacement for the more traditional experimental designs, but as an extension of them. The traditional approach has generated a vast amount of data, and is a perfectly acceptable tool in a researcher's arsenal for investigating attentional phenomena. But, because of the theoretical underpinnings of this approach, certain aspects of visual attention have been largely ignored. In this sense, an action-oriented approach can be seen to be not so much a change in the methods used *per se*, but the assumptions made on the nature of visual attention and the cognitive system as a whole. This in turn affects the type of task that the researcher chooses to study, and the applicability of the results they obtain. There is nothing new in this. A cursory glance through a publication such as the journal *Perception*, for example, will reveal a variety of theoretical approaches to perception. While the explanations advanced and the experimental tasks used vary, the basic experimental approach does not.

An action-oriented approach would not be required, however, if the traditional approach was entirely satisfactory. As argued earlier, the action-oriented approach disagrees strongly with some of the assumptions and ensuing practices that dominate the field of visual attention. In particular, the assumption that attention is a unitary causal mechanism is seen as especially problematic. It is through this assumption that generalisations from the abstracted tasks commonly used by researchers in attention are made to real-world situations. The evidence argues against this practice.

The experiments presented in this study likewise suggest that this practice is invalid. By using a natural action task, results not predicted on the basis of previous research were obtained. If the participants in Experiment 2 ignored the presence of the distractor because it was irrelevant to the successful completion of the task, why do experiments using alphanumerical stimuli presented on a computer screen not also show flat response times? After all, it would seem that the distractor in these studies is just as irrelevant to a correct response as it was in Experiment 2. This highlights the possible dangers of generalising from these studies to more real-world situations.

What is being argued here is not that researchers forego the generalisation of the results from their experiments, nor that they should refrain from speculation on the nature of various aspects of attention, but that a more conservative approach should be taken. Scientific progress relies on being able to determine the common causal mechanism underlying apparently diverse phenomena, and generalisation of results is an important part of this process. But broad generalisability can not be assumed as it has been in visual attention. Rather, generalisation has to be informed by what is known of the way perceptual information is processed.

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Appendix A: Statistical Analyses

The Binomial Expansion

To determine the p values associated with a certain number of participants conforming to a prediction, the Binomial Expansion was used (Pagano 1981; Walker 1985). This takes the form general form

$$P(X) = {}^{N}C_{X}\, p^{X} q^{(N-X)} \qquad (1)$$

where X = number of participants conforming to the prediction,

p = probability of the predicted outcome for a participant,

q = probability of the other possible outcomes (1- P),

and N = number of trials (in this case, participants)

P was arrived at by dividing 1 by the number of possible outcomes. Thus, for Study 1 P would equal 0.25, since there are four possible mutually exclusive outcomes[1]. Any of the No Distractor (0.25), Same Row (0,25), Back Row (0.25) or Front Row (0.25) conditions could be the fastest. Therefore, the probability of the No Distractor condition being the fastest is 0.25, while the probability of any of the other conditions being fastest is 0.75 (0.25 + 0.25 + 0.25). The number of participants can be seen as independent trials, and therefore constitute N. So the probability of X participants out of 14 conforming to the ND < ALL prediction in Study 1 is specified by:

$$P(X) = {}^{14}C_{X}\,(0.25)^{X}(0.75)^{(14-X)} \qquad (2)$$

Since the predictions are directional, however, the sum of the probabilities from X to the extreme in that direction needs to be calculated. So if $X = 3$, the probability of 3 or less participants conforming to the prediction is determined by:

$$P(3 \text{ or less}) = P(0) + P(1) + P(2) + P(3) \qquad (3)$$

This is the probability that is reported in the results.

[1] While it is possible that two conditions could tie as fastest equal, this never actually happened.

Experiment 2

ANOVAs

Below are tables depicting the results of the mixed design ANOVAs performed on the time measures obtained for each location.

Table 9: Mixed ANOVA Results for the Front Right Position.

° significant at the 0.10 level.
* significant at the 0.05 level

Measure	Effect	df	F =	p <
RT	Instruction	1, 12	1.06	0.324
	Distractor Position	2, 24	1.67	0.210
	Instruction x Distractor Position	2, 24	2.40	0.113
MT	Instruction	1, 12	0.13	0.729
	Distractor Position	2, 24	0.00	0.998
	Instruction x Distractor Position	2, 24	1.58	0.227
Total	Instruction	1, 12	0.01	0.914
	Distractor Position	2, 24	0.13	0.878
	Instruction x Distractor Position	2, 24	0.03	0.968
ITI	Instruction	1, 12	5.43	*0.038**
	Distractor Position	2, 24	26.37	*0.000**
	Instruction x Distractor Position	2, 24	0.00	0.183

Table 10: Mixed ANOVA Results for the Front Left Position.

° significant at the 0.10 level.
* significant at the 0.05 level

Measure	Effect	df	F =	p <
RT	Instruction	1, 12	1.38	0.263
	Distractor Position	2, 24	0.92	0.414
	Instruction x Distractor Position	2, 24	0.04	0.966
MT	Instruction	1, 12	0.14	0.717
	Distractor Position	2, 24	1.55	0.233
	Instruction x Distractor Position	2, 24	0.70	0.506
Total	Instruction	1, 12	0.01	0.932
	Distractor Position	2, 24	1.32	0.287
	Instruction x Distractor Position	2, 24	0.41	0.667
ITI	Instruction	1, 12	1.75	0.377
	Distractor Position	2, 24	12.46	*0.000**
	Instruction x Distractor Position	2, 24	3.73	*0.039**

Table 11: Mixed ANOVA Results for the Back Right Position.

° significant at the 0.10 level.
* significant at the 0.05 level

Measure	Effect	df	F =	p <
RT	Instruction	1, 12	1.11	0.313
	Distractor Position	3, 36	1.88	0.151
	Instruction x Distractor Position	3, 36	1.04	0.387
MT	Instruction	1, 12	0.55	0.474
	Distractor Position	3, 36	0.84	0.484
	Instruction x Distractor Position	3, 36	0.35	0.788
Total	Instruction	1, 12	0.06	0.818
	Distractor Position	3, 36	1.10	0.362
	Instruction x Distractor Position	3, 36	0.15	0.931
ITI	Instruction	1, 12	0.57	0.466
	Distractor Position	3, 36	2.78	*0.055°*
	Instruction x Distractor Position	3, 36	0.30	0.823

Table 12: Mixed ANOVA Results for the Back Left Position.

° significant at the 0.10 level.
* significant at the 0.05 level

Measure	Effect	df	F =	p <
RT	Instruction	1, 12	0.44	0.520
	Distractor Position	3, 36	0.14	0.936
	Instruction x Distractor Position	3, 36	1.24	0.311
MT	Instruction	1, 12	0.81	0.387
	Distractor Position	3, 36	0.39	0.763
	Instruction x Distractor Position	3, 36	0.57	0.636
Total	Instruction	1, 12	0.27	0.611
	Distractor Position	3, 36	0.31	0.815
	Instruction x Distractor Position	3, 36	0.45	0.719
ITI	Instruction	1, 12	1.38	0.263
	Distractor Position	3, 36	7.84	*0.000**
	Instruction x Distractor Position	3, 36	0.78	0.512

Post-Hoc Analyses

The table below list the results from a series of Tukey's HSD tests on a significant interaction effect between instruction and distractor position in the front left location for Inter-Trial Interval. The alpha level used was 0.05.

Table 13: *p* values obtained for the interaction effect obtained between instruction manipulation and distractor position in the front left location for the Inter-Trial Interval using Tukey's HSD Post Hoc Test.

° significant at the 0.10 level.
* significant at the 0.05 level

		Avoid			Ignore		
		No Distractor	Same Row	Back Row	No Distractor	Same Row	Back Row
Avoid	No Distractor	-	0.006*	0.970	0.871	0.725	0.970
	Same Row	0.006*	-	0.001*	0.070°	0.124	0.001*
	Back Row	0.970	0.001*	-	0.436	0.287	1.000
Ignore	No Distractor	0.871	0.070°	0.436	-	1.000	0.436
	Same Row	0.725	0.124	0.287	1.000	-	0.287
	Back Row	0.970	0.001*	1.000	0.436	0.287	-

Post-Hoc Power Analyses

Post-Hoc Power Analyses were performed to determine the statistical power of the Study for a medium effect size at a 0.10 alpha level. Table 14 summarises the results of these analyses. Note that due to the different number of distractor position conditions for when the target was on the front or back row, power analyses were required for both rows.

Table 14: Results of power analyses performed for Study 2.

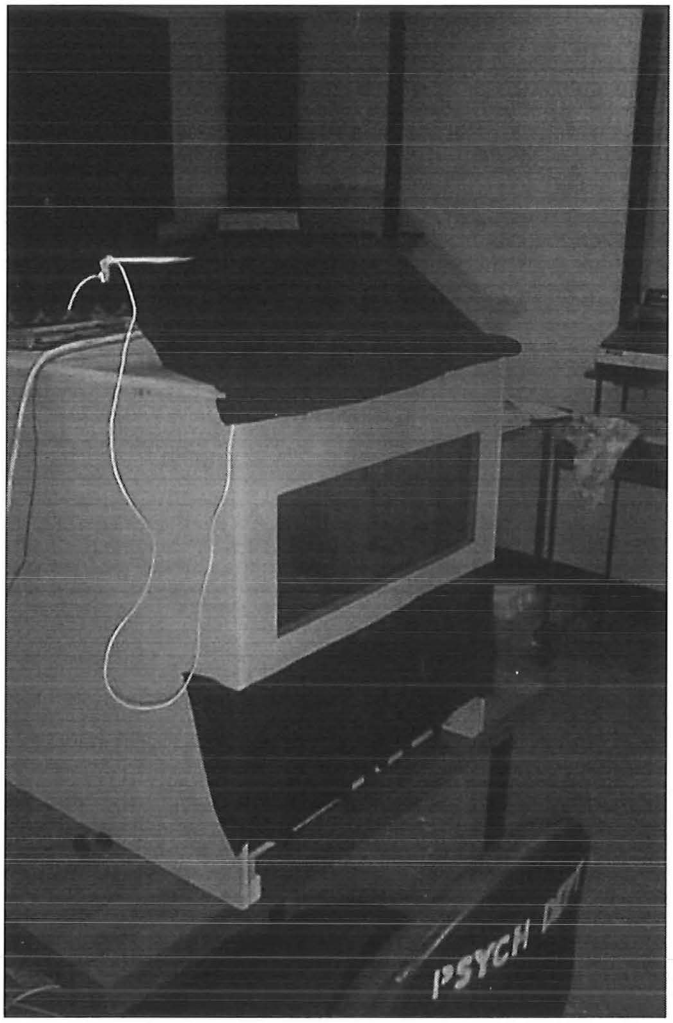
Effect	df	Lambda	Power
Instruction	1, 12	1.050	0.2535
Distractor Position (Front Row)	2, 24	1.050	0.2123
Distractor Position (Back Row)	3, 36	1.050	0.1921

Appendix B: Computer Program Information

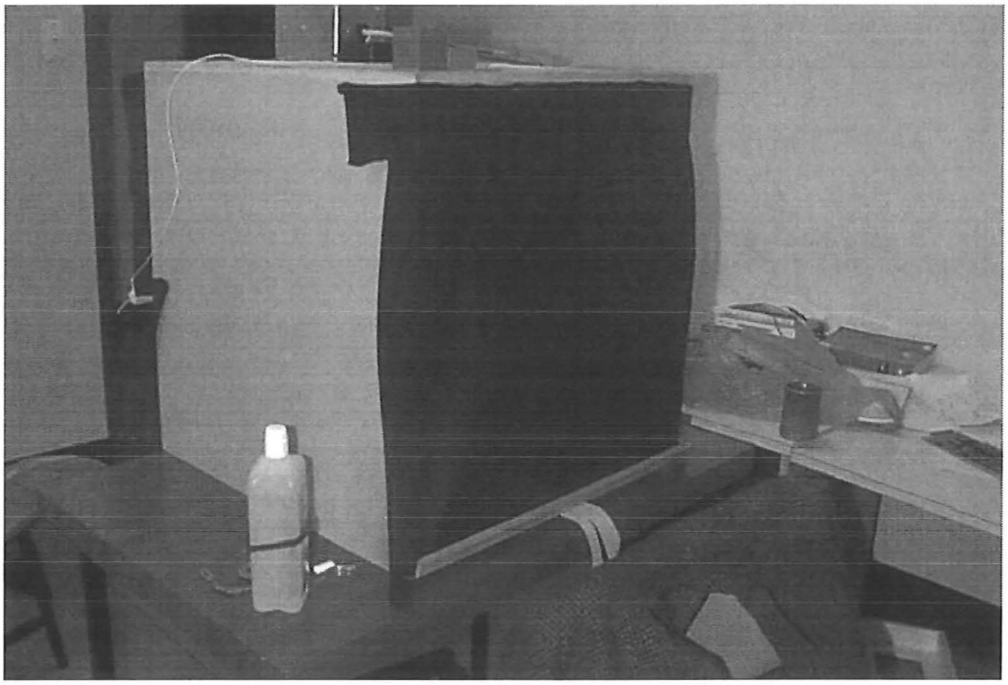
The programs used in this thesis were written in Pascal and x86 Assembler. The source code for these programs are included on the diskette enclosed inside the back cover, along with the compiled executables. The DOS executables were written using Turbo Pascal 7.0 and Turbo Assembler 5.0 by Borland International Inc. Trial generation and conversion programs were written using Virtual Pascal for OS/2 Beta 3 and Version 1.1 by fPrint Associates. All programs were developed and tested under OS/2 Warp v3 by IBM Corporation. The time-critical experimental control programs were run under MS-DOS 6.22 and 7.00 by Microsoft Corporation, with no TSRs or memory managers other than HIMEM.SYS and DOSKEY loaded. All programs were compiled with debugging information, overlay capabilities and numeric co-processor emulation switched off.

Some of the routines used were drawn or derived from the public domain SWAG Pascal source code archive (http://www.gdsoft.com/swag/). Where this has occurred, the appropriate acknowledgment has been made in the source code.

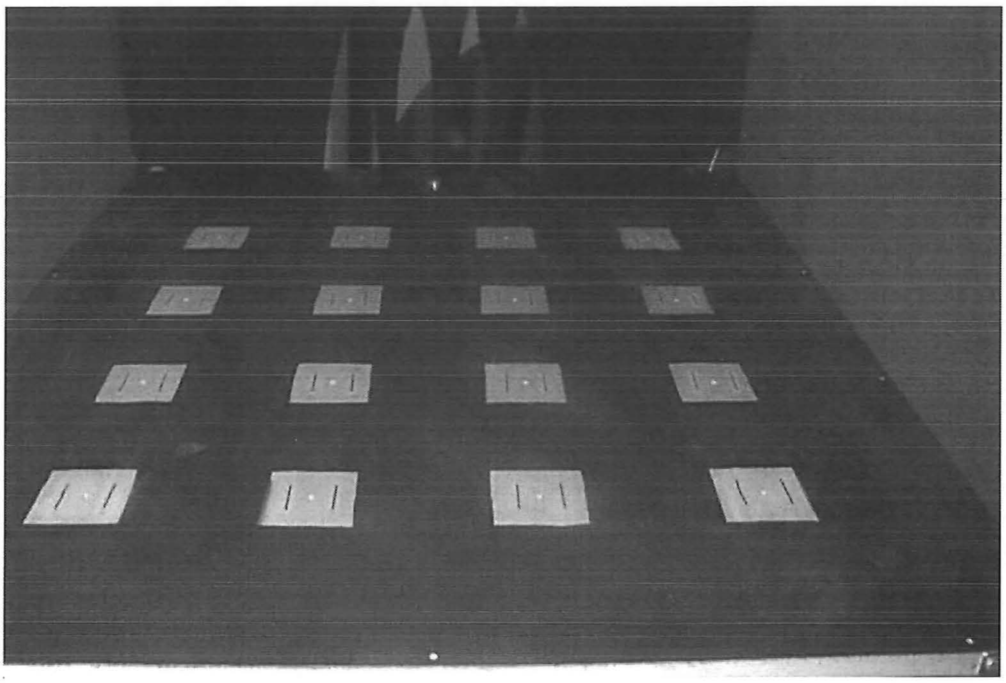
Appendix C: Photographs of the Experimental Apparatus



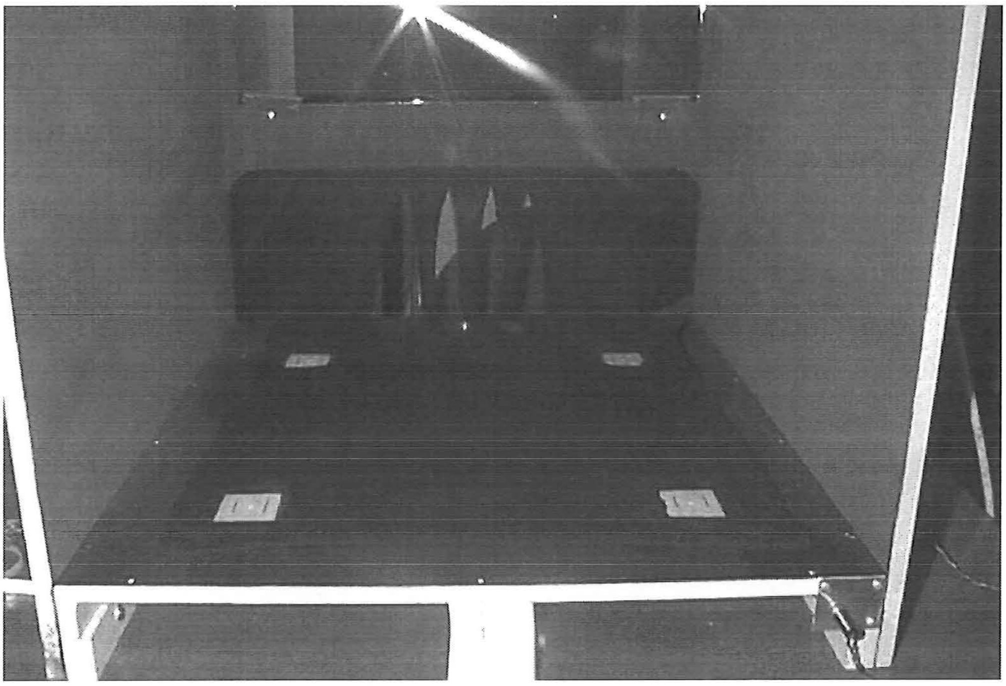
The apparatus from the front.



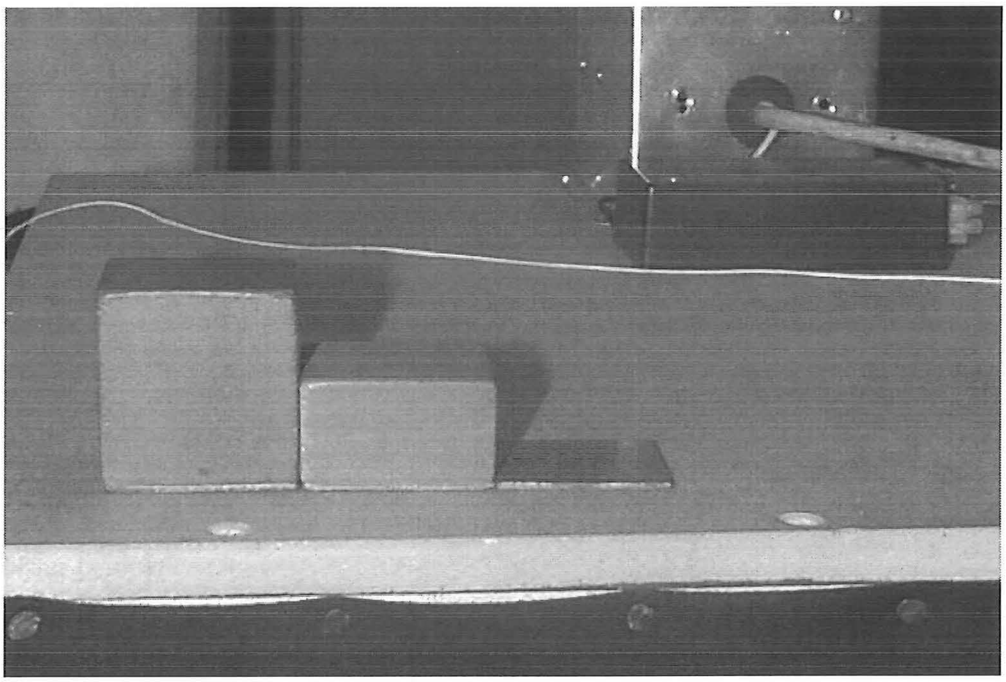
The apparatus from the rear.



The array of locations visible in Experiment 1 (from rear)



The array of locations used in Experiment 2 (from rear)



The objects used the experiments: (from left to right) The red cube, the green cube, and the copper plate painted green